

Discussion Paper and Working Paper Series

A Century of American Economic Review

Benno Torgler and Marco Piatti*

Working/Discussion Paper # 266

February 2011

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JEL Classifications: A10, A110, B00, B31, B40, I23, N01, J00, Z00

Keywords: *American Economic Review*, publishing economics, rankings, cooperation, authors, editors, board members, referees, connections, awards, paper characteristics, economic history, history of economic thought

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A Century of *American Economic Review*

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^{*}For this article, a third party tossed a coin to determine the order of the authors' names. Both are affiliated with the School of Economics and Finance, Queensland University of Technology, GPO Box 2434, Brisbane, QLD 4001, Australia. Benno Torgler is also affiliated with CREMA—Center for Research in Economics, Management and the Arts (Switzerland); CESifo (Germany); and the National Centre for Econometric Research (Australia). For helpful comments, suggestions, and excellent research support, thanks are due to Ho Fai Chan. For outstanding and detailed comments and suggestions, thanks are due to Julia Angelica and Alison Macintyre. Please address all correspondence to Benno Torgler (benno.torgler@qut.edu.au).

THE TRAGEDY OF FAUST

PART I

FAUST

...And here I stand,

With all my lore,

Poor fool, no wiser than before.

1. Introduction

Scholarly journals play a crucial role in the communication of scientific knowledge and new ideas. As Anne-Wil Harzing (author of *The Publish or Perish Book*) points out in a 2002 article, “[a]cademics and practitioners alike turn to academic articles as a reliable source of information” (127). Recently, however, the academic system has become even more competitive, producing a situation in which “graduate students and assistant professors are under extreme time pressure. In some cases, it is an ‘all or nothing’ issue: either they are able to publish in a good refereed journal, or they have to bury their dream of an academic career” (Bruno S. Frey 2005: 178).¹

This paper takes a very close look at the *American Economic Review (AER)*, the first journal of the American Economic Association. Created in 1911, *AER* is one of the top economics journals and has substantially influenced the economics landscape over the last 100 years. Economics journals began emerging a century after Adam Smith’s 1776 publication in Europe of *An Inquiry into the Nature and Causes of the Wealth of Nations*

¹ In February 2011, Maastricht University’s School of Business and Economics ran the following ad in JOE (Job Openings for Economists) for a tenured full professor: “Applicants should have proven their high research quality through publications in high-ranked peer reviewed journals. The ideal candidate will have published in the top-5. The successful candidate is expected to continue publishing in high-ranked journals. In addition, the candidate is expected to actively engage in securing research grants on a national and international level. The successful candidate is expected to be willing to teach in the area of economic theory and behavior at the undergraduate and graduate level. The language of instruction is English. The successful candidate is also expected to actively participate in the self-administration of the department” (see http://www.aeaweb.org/joe/listing.php?JOE_ID=201102_395195).

(Arthur W. Diamond 1988). Previously, serious economics were covered only in books or nonspecialist periodicals. The first fully professional journal in the U.S., the *Quarterly Journal of Economics*, came into being in 1886 (George J. Stigler, Stephen M. Stigler, and Claire Friedland 1965). Two additional U.S. publications that today are leading journals in the field were founded within the next 15 years (*AER* and then the *Journal of Political Economy* in 1892). However, as Stigler, Stigler, and Friedland (1995: 332) note, the “birth of the English-language journals slightly lags the development of economics as an academic calling. In the 1890s, there were perhaps a half dozen professorships of economics in Britain and probably no more than two or three dozen in the United States.” Interestingly, at his testimonial dinner (“Remarks” 1941), the first *AER* editor Davis Rich Dewey explained that when he was invited to undertake the managing editorship of the *American Economic Review*, he “demurred, partly on the grounds that I did not read easily foreign languages. The chief qualification I possessed was that I had edited the quarterly of the American Statistical Association during its first fifteen years. Fortunately, French and German statistical tables could be easily deciphered. It was far otherwise with the journals conducted by Schmoller and Conrad. I was, however, assured that the Association wished to publish an *American* economic journal and that my objection was not an outstanding defect” (viii).

The first article in *AER*, entitled “Some Unsettled Problems of Irrigation,” was published in 1911 by a distinguished female economist, Katharine Coman (1857–1915), who in the early 1880s, was the first American woman to become a professor of statistics (Gerard F. Vaughn 2004). During the early 1900s, she also chaired the Department of Economics and was Dean of Wellesley College. Three papers in the first *AER* issue in 2011 were dedicated as commentaries to Coman’s contribution (see Elinor Ostrom 2011, Gary D. Libecap 2011,

and Robert N. Stavins 2011).² According to Dewey, the inaugural issue led to no end of trouble (as quoted in Ann Mari May and Robert W. Dimand 2009), a situation that has been referred to as the Cross/Eaves Controversy.³

Davis Rich Dewey was in charge of managing *AER* for 30 years (between 1911 and 1940), a longer tenure than any other editor since. In the same testimonial dinner speech, he emphasized his situation at the time: “When I assumed the duties of managing editor, I was past middle age and had settled down to the routine of teaching undergraduate students. I had no graduate students with whom I could soar into distant stratospheres where dynamic principles seem to contradict those which operate upon this lowly earth. Although I had no competency for empyreal flights, I enjoyed and even envied the tales which these aeronautical economists, with their helical ascents, brought back. A few of these are nose-divers as well, and I read with some satisfaction that these nose-divers, in pulling out from the dive, have a rush of blood from the head to the feet and the flyer often blacks out into unconsciousness. In this way you supplemented the pleasures of my later life; and for this I again give you my most hearty thanks” (“Remarks” 1941: vii).

One key task of a new journal is to distinguish itself from those journals already in existence. In addressing this task, Dewey points out, he “had no clear answer, but believed

² Elinor Ostrom (2011: 49), for example, points out that “Coman described tough collective-action problems half a century before Mancur Olson (1965) and Garrett Hardin (1968: 49) identified the challenging theoretical problem facing many groups.” Likewise, Gary D. Libecap (2011: 64) stresses that “[t]hese same issues have relevance today, 100 years later, in the face of growing concern about the availability of fresh water world-wide as demand grows and as supplies become more uncertain due to the potential effects of climate change.” Robert N. Stavins (2011: 81) echoes these sentiments by beginning his article as follows: “As the first decade of the twenty-first century comes to a close, the problem of the commons is more central to economics and more important to our lives than a century ago when Katharine Coman led off the first issue of the *American Economic Review* with her examination of ‘Some Unsettled Problems of Irrigation’ (Coman 1911). Since that time, 100 years of remarkable economic progress have accompanied 100 years of increasingly challenging problems.”

³ A critical review by Ira Cross of a book by Lucile Eaves was the starting point for this controversy. It prompted a reaction from Eaves, who saw it as an unjust attack and mobilized a large number of economists in her defense. This reaction led to a debate on the length of a reply. After a shortened form was published, Cross exploded because he did not receive an advance copy of Eaves’ response. He then sought the chance to submit a reply in order to have the last word. Dewey was then bombarded with letters from Eaves expressing her opinion (for a detailed discussion see Ann Mari May and Robert W. Dimand 2009).

that, if possible, the new Review should serve as a working tool for the hundreds of graduate students who were on their way to becoming teachers of economics; and secondly that the Review should be the organ of the members of the Association rather than the organ of the editors” (“Remarks” 1941: ix).

Since then, *AER* has had 9 additional editors: Paul T. Homan (1941–1951),⁴ Bernard F. Haley (1952–1962), John G. Gurley (1963–1968), George H. Borts (1969–1980), Robert W. Clower (1981–1985), Orley Ashenfelter (1985–2001), Ben S. Bernanke (2001–2004), Robert A. Moffitt (2004–2010), and Pinelopi Koujianou Goldberg (2011–). Goldberg, who received her Ph.D. from Stanford University in 1992 and is the William K. Lanman, Jr. Professor of Economics at Yale University, is the first female editor⁵ in *AER*’s 100-year history, after having been its coeditor between 2007 and 2010. Interestingly, the Johns Hopkins University appears both at the beginning and the end of *AER*’s century. Robert A. Moffitt (editor from 2004 to 2010) is the Krieger-Eisenhower Professor of Economics at the Johns Hopkins University and has worked at the university since 1995. Davis Rich Dewey was a student at Johns Hopkins of Richard T. Ely,⁶ one of the key driving forces in establishing *AER*: “For three years I had been a pupil of Dr. Ely at Johns Hopkins University, and it could not be expected that I would reject the blandishments of my enthusiastic teacher. I was a trailer rather than a founder” (“Remarks” 1941: viii).

⁴ Paul Homan moved several times between Ithaca, Washington, and London before settling in Los Angeles in 1950. As A. W. Coats (1969: 63) points out, “the first of these moves aroused some misgivings in the minds of his Association colleagues, who feared that the *A.E.R.* would be neglected; and his work in London necessitated the appointment of Fritz Machlup (who, like Homan, was working in Washington) as Acting Managing Editor from July, 1944 to August, 1945.”

⁵ The notation has changed over time (from managing editor, to production editor, back to managing editor, and again to editor (see Robert A. Margo 2011: 17).

⁶ Coats (1960: 560) provides a useful overview of Ely’s contribution to the American Economic Association and points out that “despite the danger of overestimating the role of a single individual in the life of an organization, it is no exaggeration to say that in the early years of its history, the public response to the American Economic Association was largely determined by the various reactions to his work.” Ely, described as “a vigorous young member of The Johns Hopkins University,” was the initiator of the official inauguration of the American Economic Association on September 9, 1885 (555).

This present paper, however, does *not* aim to provide a detailed historical overview of *AER*, not least because A. W. Coats (1969) has developed an excellent historical account of *AER*'s pre-history and the first period of the journal under the editorial regimes of Dewey, Homan, and Haley (1911–1962). Coats' analysis has also been recently updated and extended by Robert A. Margo (2011) to cover the period from the 1970s to the present. Margo's account, however, rather than focusing on the editors, explores the expansion of the economics profession and documents, in three figures and two tables, such statistics as the ratio of published-to-submitted manuscripts (1948 –2006) and the average length of the articles for various years.⁷ He also conducts a citation analysis of articles published in *AER* from 1960 to 2000⁸ which, interestingly, reveals that “elite citations”⁹ are quite infrequent but did increase over time (influenced by an increase in the 1960s and 1990s). However, he notes: “Although elite citations for the average *AER* paper were higher for the 2000 cohort than for the 1970 cohort, this is *not* true if the rates are page standardized. That is the rise in average length is the mechanism through which the increase frequency of elite citation was realized historically. Prospect authors may shudder at the (very) low unconditional acceptance rate, but subscribers can take some comfort in the knowledge that today's *AER* is better than their father's *AER* in this (admittedly limited) sense” (Margo 2011: 32).

Given that these historical overviews of *AER* have already dealt with the journal's development, we have discretionarily selected the topics for this paper based on what we ourselves find interesting and what we hope will prove enjoyable for our readers. The selection does not, therefore, aim to provide coherence or historical structure, which lightens

⁷ Specifically, 1950, 1960, 1970, 1980, 1990, 2000, and 2005. Interestingly, there was a 68% increase in length between 1980 and 2005.

⁸ At five-year intervals, based on age, cohort, and period effects. As Margo (2011:35) points out, “[k]nowledge advances and thus scientific papers have a shelf life. Advances come in clumps, editors come and go, and other factors may make a particular year's papers unusually significant or unusually stale.”

⁹ That is, citations appearing in other *AER* articles or citations to *AER* articles in the top-6 leading economics journals (*AER*, *JPE*, *QJE*, *Review of Economics and Statistics*, *Review of Economic Studies*, and *Econometrica*). Margo (2011: 32) stresses that “if some *AER* publication cohorts were really ‘better’ on average than others—this should be reflected in a greater likelihood of citation in the top journals.”

the burden of ensuring completeness and helps us maintain a high level of intrinsic motivation. Indeed, as George Loewenstein points out in the introduction to his book *Exotic Preferences* (2008: xv), “the overwhelming desire to include everything is one of the most destructive motivations in academia, and it applies not only to seminars, but to the choice of chapters for a book of collected works like this one.”¹⁰ As a result, our contribution merely scratches the surface and presents only a sketch—one however, that we hope is deep or interesting enough to attract the attention of readers in an academic environment suffering from serious congestion on the supply side.¹¹ We also somehow hope that there is something for everyone in this contribution.

In the spirit of Dewey, this work is perhaps more of a statistical than an economics paper.¹² That is, it is descriptive rather than causal and retrospective rather than forward looking. That is, we make little attempt to put forward a mechanism driven by hypotheses that provide a coherent *overall* description of *AER*’s development¹³ and report rather than synthesize what we believe is interesting in the journal’s history. We would also like to apologize in advance for the copious use of quotes. Having chosen no overall theory on which to base our analysis, we felt it appropriate in a descriptive paper to draw extensively on

¹⁰ The length of this paper shows that we have partially failed to control such forces.

¹¹ Sixteen years ago, George J. Stigler, Stephen M. Stigler, and Claire Friedland (1995: 333–334) asked the following question: “How many can one examine of the two to four dozen journals that carry papers one sometimes wishes to read? Of course there are bibliographic services such as the listing of economic articles in the *Journal of Economic Literature*, but the very length of these listings is another form of congestion.”

¹² A. W. Coats (1969: 59), referring to an interaction between Dewey and H. G. Moulton, points out that by “training and predilection Dewey was a statistician and historian rather than an economist, and he made no effort to conceal the limitations of his theoretical equipment. While not hostile to theory, he assigned it a subordinate role, believing that priority should be accorded to the economist’s ‘sacred obligation to handle facts with respect.’”

¹³ The potential does exist to reduce such shortcomings. In his brilliant book *The Nature of Creative Development*, Jonathan Feinstein (2006: 28) suggests a way of presenting a model as a causal structure using “language in which creative interests and conceptual structures in interest domains are described as generative of creativity and guiding individuals in their development.” The causal mechanisms he describes are “not proximal, but extend over longer time periods” and, as hypotheses, function to “generate a coherent description of creative development.” One other methodology that could be considered is analytic narratives, in which researchers “attempt to address several issues. First, many of us are engaged in in-depth case studies, but we also seek to contribute to, and to make use of, theory. How might we best proceed? Second, the historian, the anthropologist, and the area specialist possess knowledge of a place and time. They have an understanding of the particular. How might they best employ such data to create and test theories that may apply more generally? Third, what is the contribution of formal theory? What benefits are, or can be, secured by formalizing verbal accounts?” (Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry Weingast 2000)

past descriptions. We also thought that the paper needed a personal touch,¹⁴ although we are aware that this may (somewhat ironically) support the cult of personality, in particular for contributions that are cited extensively (e.g., the work of Paul A. Samuelson and Bruno S. Frey). Awareness of this irony, however, allows us to eventually derive policy implications. That is, with the help of the data, we try to provide the right camera angles, lens effects, and lighting. We also try to script the quotes' character development so that they optimally shape the paper while allowing us to discuss certain *AER* publications in greater detail. These latter have also been selected in a relatively discretionary manner so that we may relate them to the topics explored.

While putting together this contribution, we avoided the task of thinking about boundaries (e.g., space restrictions) so that we were free to work for our own approval.¹⁵ The ability to publish a contribution as a working paper alleviates the pressure to publish it as an article or to think strategically about producing text in which a referee will find less to criticize.¹⁶ Thus, providing there is no refereeing system similar to that of a printed journal, it lightens the burden of being forced to intellectually prostitute oneself (Frey 2005).¹⁷ In our case, we believe that this lack of restrictions has been beneficial and has facilitated a relaxed approach to exploring the topics in this paper.¹⁸ We also recognize, however, that one may pay a price if the research product is not user friendly. Academia in general has developed into a "battle of attention" based on an overflow of papers produced per year, and economics

¹⁴ Margit Osterloh and Bruno S. Frey (2009), for example, refer to a study that shows that around 70 to 90% of the papers cited have not been read and have been copied from the list of references used in other papers.

¹⁵ However, this does not exclude the idea put forward by Paul A. Samuelson (2004: 61), that "part of this involves the faith that what one craftsman will like, so will eventually the rest."

¹⁶ Bruno S. Frey (forthcoming: 4) discusses publication pressure: "An effort must be made to 'sell' the results as novel and pertinent, although there is an incentive to suppress the works and insights of other scholars who are unlikely to be the editors or the referees of the journal in which one aspires to publish."

¹⁷ For example, citations reduce the importance of journal publications as citations can refer to any work available to researchers such as books, book chapters, or working papers (Bruno S. Frey 2005).

¹⁸ "If scholars are to be original in a similar way to artists today, they have to be given as much independence as possible. This principle has been accorded to universities a long time ago, and is rightly guarded by them. Nevertheless, economics scholars have managed to establish a journal publication system, tending to turn them into intellectual prostitutes" (Frey 2005: 182).

is no exception. Such proliferation is amply illustrated by the development of IDEAS, the largest bibliographic database dedicated to economics and freely accessible on the Web, which aims to enhance the free dissemination of research in economics (see <http://ideas.repec.org/>). This repository contains information on 11,975 institutions, covering 26,852 authors registered with the RePEc Author Service who have authored 593,619 items listed in the archive. In 2010, the information dissemination service NEP (New Economics Papers) sent out 4,448 weekly reports about new research based on 87 fields, and the RePEc services recorded 8,989,727 downloads and 30,777,612 abstract views (see <http://blog.repec.org/>, accessed January 6, 2011). Not only does this level of activity seem to characterize a highly competitive environment, but academics cannot assume or expect that fellow researchers will read a paper that does not use what George J. Stigler (1955: 295) terms the “technique of persuasion.” He explains this technique as follows: “Suppose I get a ‘new’ idea and gradually work it (and myself) into the state where I believe that it is scientifically important. I may then find it in earlier writers, for now I am sensitive to the subject and look more searchingly and sympathetically at the relevant writing. Then I take up my pen, and write: I submit for critical appraisal my recent thoughts on linear oligopoly. These thoughts, which were first presented by Süssmilch in 1745, by Say in 1814, and then became so common as to reach the United States by 1870, are by no means of major significance. In fact if they are improperly utilized they will probably lead to serious error. Yet under certain conditions, which I am not fully able to specify, they offer promise of a minor usefulness. Will my fellow economists read on—once they get used to the novelty of this approach? No. New ideas are even harder to sell than new products. Inertia and the many unharmonious voices of those who would change our ways combine against the balanced and temperate statement of the merits of one’s ‘original’ views. One must put on the best face possible, and much is possible. Wares must be shouted—the human mind is not a divining

rod that quivers over truth.” How, then, can one achieve persuasion? Stigler (1955: 295) provides the following answer: “The techniques of persuasion also in the realm of ideas are generally repetition, inflated claims, and disproportionate emphases, and they have preceded and accompanied the adoption on a large scale of almost every new idea in economic theory. Almost, but not quite, every new idea. A few men have such unusual powers that their contemporaries recognize their claims without the usual exaggerations: Smith and Marshall are the only economists who seem to me indisputably to belong in this supreme class.”

To generate our datasets, we primarily accessed resources that should be available to most economists,¹⁹ academics or not, given that AEA membership provides access to the association’s journals and to JSTOR.²⁰ Most particularly, this choice is consistent with the fact that *AER* is an official AEA journal²¹ rather than the product of an economics department or institution: “In the end, the *American Economic Review* was launched in 1911 mainly because a substantial body of rank and file members was determined to assert the Association’s right to its own journal” (Coats 1969: 58). We hope, therefore, that our selection will allow others to easily replicate our work or move beyond our analysis.²²

¹⁹ We work mainly with *AER* articles or articles discussing *AER* contributions, annual reports of the (managing) editor, and occasionally *Journal Citation Reports*.

²⁰ There are restrictions, however, for those interested in constructing a similar database. We have been informed that the JSTOR system has a number of tools in place that monitor excessive downloading and other activities that contravene the JSTOR Terms and Conditions (<http://www.jstor.org/page/info/about/policies/terms.jsp>). When such excessive downloading takes place, their system automatically denies JSTOR access from the IP address for a short period of time in order to disrupt this activity. If the activity persists, JSTOR will then contact the license holder (such as the library), providing details of the downloading so that there is an opportunity to shut down the accounts responsible as soon as possible.

²¹ Stigler, Stigler, and Friedland (1995: 346) refer to it as the “flagship journal of the major North American economic society.”

²² In the spirit of a 1986 editorial statement that appeared in vol. 76, no. 4 of the *American Economic Review* under Orley Ashenfelter’s leadership as managing editor (with Robert H. Haveman, John G. Riley, and John T. Taylor as co-editors), we will make our datasets and (mostly STATA) estimations accessible: “It is the policy of the *American Economic Review* to publish papers only where the data used in the analysis are clearly and precisely documented, are readily available to any researcher for purposes of replication, and where details of the computations sufficient to permit replication are provided. The Managing Editor should be notified at the time of submission if the data used in a paper are proprietary, or if, for some other reason, the above requirements cannot be met” (v). In 2004, *AER* “began to require that authors of accepted papers that employ data in econometric exercises, simulation models, or experiments agree to post their data and programs on the journal Web site unless an exemption for proprietary data is requested and granted. The policy was strengthened in 2005 with more systematic enforcement and with tighter rules for exemptions. Exemptions are generally

Clearly, such an approach has substantial shortcomings. For instance, we are unable to explore many interesting questions that would require the cooperation of the journal itself (through the release of detailed data on submissions, assigned referees and editors, and so forth). With access to this information, we could have analyzed such topics as the determinants of success in publishing in *American Economic Review*, a subject of interest to many researchers in our economics guild. Admittedly, however, doing so would also raise the challenging question of which came first, the chicken or the *golden egg*.²³

2. Top Institutions, Top Papers, and Leading Economists Publishing in AER

Given that there appears to be a natural desire for distinction (see, e.g., the work of Robert Frank²⁴), it is no surprise that a large set of papers has emerged on the rankings of economics departments and researchers (see table 1) and that the last two decades have witnessed an impressive development of economic research on the evaluation of scientific progress (Rabah Amir and Malgorzata Knauff 2008). The demand for rankings is understandable as academics in the market are keen to obtain information on the best research environment in which to continue their careers. University administrators also find rankings useful for evaluating the progress of their departments and for developing student recruitment strategies (Loren C. Scott and Peter M. Mitias, 1996).²⁵ More important, as pointed out by David N. Laband and Robert D. Tollison (2003), there has also been a “considerable ramping up of the incentives and imperative for faculty to be ‘research-active’”. This increased

granted only if the data can be accessed by other researchers in some alternative fashion” (Report of the Editor, *AER Papers and Proceedings*, 96(2): 498). This report presents tabular evidence (in table 8, page 501) of full compliance after a second-round reminder. Only 4 out of 99 papers were given an exemption.

²³ For a valuable and entertaining discussion on causality, see Joshua D. Angrist and Jörn-Steffen Pischke’s (2009) book *Mostly Harmless Econometrics*.

²⁴ For example, Robert H. Frank and Philip J. Cook (1996), Robert H. Frank (1985, 1991, 1999), and Robert H. Frank and Cass R. Sunstein (2001).

²⁵ However, Osterloh and Frey (2009) point that output-oriented rankings can crowd out intrinsically motivated curiosity to conduct research. The danger is that such intrinsic motivation is substituted by extrinsic motivation to earn high ranking scores. Thus, content loses its relevance. These authors therefore suggest that evaluators work with a combination of qualitative peer reviews and bibliometrics (informed peer reviews).

emphasis on research is evident not only in the United States, but also in many countries in Europe, as well as Canada, Australia, and several South American countries. In the United States and Canada the principal inducements have taken the form of reduced faculty teaching loads, marginally higher raises for faculty who publish in peer-reviewed journals, and financial support for conference travel; in other countries there has been a less pronounced decrease in faculty teaching loads, but there are pecuniary rewards to publishing” (161–162). Bruno S. Frey (forthcoming: 2) argues that today, “the importance of scientific idea and the position of a scholar are defined by *rankings*. What matters nowadays is the recognition produced by a general rankings system, normally based only on the *quantity* of scientific output, irrespective of quality.”

Interestingly, researchers today invest much effort in having their contributions screened, which indicates a keen interest in publishing well. For example, in 1974, only 19% of the papers published in *AER* had been presented at one or more conferences, workshops, or seminars for critical commentary prior to publication. On average, the number of presentations was 0.24 per paper. Twenty-five years later, 73% of the papers accepted for publication had been previously presented, and the mean number of pre-publication presentations was 4.73. Similarly, the mean number of informal contributors thanked per *AER* article increased from 4.33 to 9.59 (Laband and Tollison, 2003: 165–166). To some extent, however, such a “tour de force” in the academic circle is *almost* reminiscent of a promotional tour for a best-selling book, although definitely less glamorous and time consuming and perhaps more rewarding in terms of content feedback.²⁶ This book-promoting experience is vividly illustrated by Judith Krantz’s description of her promotional tour for

²⁶ In addition, David N. Laband and Michael J. Piette (1994: 196) state that editors are searching for good papers and compete to identify them. Thus, as “part of this competitive process, editors attend professional meetings to learn about research papers in the ‘presubmission’ stage. Many universities housing journal editorships maintain active seminar series that become de facto, and valuable, inputs in this search process.”

Scruples.²⁷ “Touring for a book—it’s the literary equivalent of war. I remember my hardcover tour. I’d hit a city—say, Cleveland—at night, unpack, steam out the clothes that were wrinkled, and, the next morning, get up at six. Because there’s always an ‘A.M. Show,’ a ‘Good Morning Show,’ a ‘Hello Show’ in every city in the country.(...) When you leave that hotel early in the morning, you have to be packed up and all checked out—the publisher has a limo to get you to the studio, and your suitcase is going to be in that limo all day while you make your sixteen different stops. Your arrival at the studio is at seven-thirty or eight, and the author invariably goes on last, but you have to be there an hour ahead of time in order to keep them from going crazy. Then, after I went on, I’d do a whole day of media in Cleveland, finishing up at six o’clock, just in time to catch a plane to Detroit, and the departure gate is *always* at the very end of the airport. You do all that day after day and enough weeks in a row, and you get so that you feel you can hardly function” (as quoted by Robert H. Frank and Philip J. Cook 1996: 9–10).

2.1 Institutional Rankings

In assessing institutional rankings, to obtain what Frey (forthcoming: 9) describes as an aggregate “super ranking,”²⁸ we first aggregated the institutional ranking results presented in several previous journal articles.²⁹ To generate table 1, which summarizes our aggregations, we looked at all the tables in these articles and counted the number of times a university

²⁷ This book, her first novel, published in 1978, when Krantz turned 50 (see Wikipedia http://en.wikipedia.org/wiki/Judith_Krantz), reached the number one spot on the *New York Times* bestseller list. As Krantz described it, “[j]ust in time for my 50th birthday, I discovered that I could write fiction. My husband had urged me to try fiction for 15 years before I did. (...) I believed that if I couldn’t write ‘literature,’ I shouldn’t write at all. (...) Now, I would say to young women, do something you have a true feeling for, no matter how little talent you may believe you have. Let no masterwork be your goal—a modest goal may lead you further than you dream” (see <http://www.wellesley.edu/Anniversary/krantz.html>).

²⁸ Frey (forthcoming: 9) discusses the direction in which academia could be heading: “The ranking mania may be expected to lead to more and more rankings, which then will be aggregated to super rankings and super rankings of super rankings (...) until it becomes clear to everyone that numerical evaluations of academic research lead to nothing, and people return to evaluate the *content* of science.”

²⁹ In many cases, we observe single rankings that accumulate a relatively large number of journals together. For example, Loren C. Scott and Peter M. Mitias (1996) used 36 journals to develop a university ranking.

appeared in the top 10 (first results column) or top 20 institutions (second results column). This table may thus provide a simple overview of institutional historical strength that takes into account the advantages and shortcomings of different ranking methods and approaches. The results in table 1 do clearly indicate the dominance of U.S. universities. More specifically, the best 15 universities are in the U.S., and among the 23 universities listed, only three are outside the U.S. In first place is MIT, followed by Harvard, and Chicago.³⁰ As two of these studies used only U.S. data³¹, we present in the appendix table A1 a revised ranking excluding these two papers. The results are very similar with only minor changes. For example, Columbia University is now in the top 10 and University of California, Berkeley has improved its position among the top 10 university.

In the next four tables, we focus entirely on single main articles that appeared over the 100 years of *AER*'s existence (1911–1920 and 1931–1940 in table 2; 1950–1959 in table 3; and 1981–1990 and 2001–2010 in table 4).³² For the 1950–1959 period only, we rely on results generated in a previous *AER* publication (Frank R. Cleary and Daniel J. Edwards 1960).

³⁰ We do not wish to offend any institution, but these results (i.e., the top three universities) “cry out” for a reference to Paul A. Samuelson’s (2004: 54-55) contribution to the *Lives of the Laureates*: “Here briefly, in the third person for objectivity, is the superficial outline of my scientific career. (...) If PAS was born as a child his freshman year at Chicago, he was born a second time as a man that October 1940 day he succumbed to a call from MIT. MIT’s force met no detectable Harvard resistance, so the movable object moved. It was the best thing that could have happened to PAS. A boy must always remain a boy in his father’s house. On his own acres a man can build his own mansion and after 1941 PAS, along with magnificent colleagues, was able to help build up what became recognized as a leading world center for economics. Living well is the best revenge, Hemingway’s crowd used to say; but, in sober truth, the example of MIT’s Norbert Wiener, who in his days of fame still brooded over his ejection from Harvard Yard, led PAS ever to cherish his Harvard connections and labor for the greater glory of Cambridge and Middlesex County.”

³¹ Richard Dusansky and Clayton J. Vernon (1998) and Loren C. Scott and Peter M. Mitias (1996).

³² When more than one author affiliation was listed, we used the author’s main affiliation.

TABLE 1: A SUMMARY OF INSTITUTIONAL RANKINGS

University	Appearance as a Top 10 University	Appearance as a Top 20 University
Massachusetts Institute of Technology	38	38
Harvard University	34	36
University of Chicago	32	36
Stanford University	31	37
Princeton University	30	36
University of Pennsylvania	28	33
Yale University	27	32
University of California, Berkeley	26	33
Northwestern University	22	32
University of California, Los Angeles	15	31
Columbia University	13	26
University of Michigan	12	26
University of Wisconsin	11	29
Carnegie Mellon University	7	17
New York University	6	25
Hebrew University	5	8
University of Washington	4	8
Rochester University	4	24
University of Western Ontario	3	10
London School of Economics	3	13
University of Minnesota	3	20
Brown University	3	10
Cornell University	3	16

Notes: Data from Tom Coupé (2003), table 2 and table 4 (covering two time periods, 1978–1982 and 1996–2001; four different rankings); two tables from Philip E. Graves, James R. Marchand, and Randal Thompson (1982), table 1 and table 2; table 1 from Richard Dusansky and Clayton J. Vernon (1998); table 3 from Pantelis Kalaitzidakis, Theofanis P. Mamuneas, and Thanasis Stengos (2003); five from Erkin Bairam (1994), table 1 (*AER* 1985–90), table 2 (*Econometrica* 1985–90), table 3 (*Economic Journal* 1985–90), table 4 (*JPE* 1985–90) and table 5 (*QJE* 1985–90); table 1 from Amir and Knauff (2008); three tables from Stephen Wu (2007), table 2 (*AER*), table 3 (*JPE*), and table 4 (*QJE*) between for the 2000–2003 period; eight from Scott and Mitias (1996), table 1 (1984–93), table 3 (1984–93), table 4, tables 5, 6, 7 (a comparison of the Top 5 in five journals); and 12 by John J. Siegfried (1994), table 1 (*AER*, by decade between 1950 and 1989), table 2 (*JPE*, by decade between 1950 and 1989) and table 3 (*QJE*, by decade between 1950 and 1989) and table 2 by Jean Louis Heck (1993).

Table 2, which focuses on the periods 1911–1920 and 1931–1940, ranks the institutions that contributed 100 pages or more to *AER*. As Paul A. Samuelson (2004:50) points out, in 1935, “only a few strong centers for economic research—Harvard, Chicago, Columbia, and a few others were visible.” Besides these three institutions, we also find names such as Princeton University, Yale University, New York University, Cornell University, University

of Illinois, or University of Wisconsin at the top. These six (1911–1920) or seven institutions (1931–1940) represent over 30% of all pages published as primary *AER* articles.³³

TABLE 2: TOP INSTITUTIONS 1911–1920 AND 1931–1940

1911–1920		
Institutions	Number of Pages	Percent of Total Pages
Yale University	324	9.2
Harvard University	254	7.2
Princeton University	228	6.5
Columbia University	215	6.1
University of Illinois	172	4.9
Cornell University	140	4
Total	1333	37.9
1931–1940		
Institutions	Number of Pages	Percent of Total Pages
Columbia University	248	6.8
Harvard University	197	5.4
Princeton University	176	4.8
University of Wisconsin	141	3.9
New York University	133	3.7
University of Chicago	130	3.6
Ohio State University	123	3.4
Total	1148	31.6

Notes: Institutions whose total contribution to *AER* during the period was 100 pages or more (based on author affiliation). Only primary journal articles are counted (i.e., *Papers and Proceedings* are excluded). Washington, D.C., although it contributed 125 pages in 1931–1940, is excluded because no affiliation was provided.

Table 3 shows the institutions whose total contribution to *AER* during the 1950–1959 period was 100 pages or more. These results reflect Samuelson’s (2004) reference to “a postwar boom in education and how the ‘expansion in economists’ prestige and self-esteem has been followed by some leaner years. We have become more humble and, as Churchill

³³ The Pax Americanus provided good U.S. institutions with a further advantage: “Hitler gave us even before the war the cream of the continental crop. (...) Along with such names as Einstein, von Neumann, and Fermi go such American economists [sic] names as Koopmans, Leontief, Schumpeter, Marschak, Haberler, Kuznets, and many others. Later, as strength draws to itself strength, there began to appear in the American lineup the names of Hurwicz, Debreu, Theil, Bhagwati, Coase, Fischer, and many others” (Samuelson 2004: 51).”

said, we have much to be humble about” (p. 52).³⁴ This expansion of the educational system led to excellent graduate departments all over the U.S.: “the period from 1932 to 1975 was a favorable one for economists like me, in that it was an epoch of tremendous university expansion and job opportunity. If one can borrow from the vulgar terminology of economic science fiction, my generation of economic activity was buoyed along by the great wave of a Kondratieff expansion” (Samuelson 2004: 50).³⁵ As table 3 shows, during the 1950–1959 period, other institutions such as the University of California, MIT, Stanford University, the Johns Hopkins University, the University of Pennsylvania, Vanderbilt University, and the Carnegie Institute of Technology appear together with the Federal Reserve System and the International Monetary Fund. Moreover, not only are the top 7 institutions responsible for around one-third of the published pages, but all 17 institutions together account for more than 60% of all the pages published in *AER*.

Table 4 provides an overview of what has happened in more recent times, with a focus on institutions whose total contribution numbers 200 pages or more. The institutions new to the list are from outside the U.S. and include the University of Western Ontario, the University of British Columbia, and the London School of Economics (LSE). In fact, the 2001–2010 data indicate that the concentration of universities at the top of the list has decreased. Whereas in the 1950s, 17 institutions were responsible for 60% of *AER* content, during the first decade of the 21st century, 18 institutions provided only 46% of the content.

³⁴ As Samuelson (2004: 52) comments, “[e]conomists have not been able to agree on a good cure for stagflation. That disillusions non-economists. And, to tell the truth, it punctures our own self-complacency. We shop around for new paradigms the way alchemists prospect for new philosophers’ stone. Just because a National Bureau paper is silly does not mean it is uninteresting. Just because it is profound does not mean that it is admired.”

³⁵ As Samuelson (2004: 52) also points out, however, “[t]rees do not grow to the sky. Every Kondratieff wave has its inflection point.”

TABLE 3: TOP INSTITUTIONS 1950–1959

Institution	Number of Pages	Percent of Total Pages
University of California	392	6.9
Massachusetts Institute of Technology	363	6.4
Stanford University	309	5.4
University of Chicago	218	3.8
University of Michigan	214	3.8
Federal Reserve System	200	3.5
Johns Hopkins University	199	3.5
University of California, Los Angeles	197	3.5
Harvard University	185	3.3
Yale University	164	2.9
University of Wisconsin	158	2.8
University of Pennsylvania	135	2.4
Princeton University	134	2.4
University of Illinois	133	2.3
Vanderbilt University	112	2.0
Northwestern University	111	2.0
Carnegie Institute of Technology	102	1.8
International Monetary Fund	100	1.8
Total	3426	60.5

Notes: Institutions whose total contribution to *AER* during the period was 100 pages or more (based on author affiliation), but *Papers and Proceedings* are excluded. Data from this time period are from Cleary and Edwards (1960: 1012). A contribution was defined to include articles, review articles, notes, communications, and memorials; book reviews are excluded.

We now adopt a different perspective and investigate the publication record based on where authors obtained their doctoral degree rather than their affiliated institution. We take a closer look at the last 60 years and analyze the data using shorter time periods (5 instead of 10 years; the 1984–1988 and 2004–2008 periods). These results reveal a much stronger concentration: researchers with a Ph.D. from the 10 universities listed in table 5 are responsible for more than 70% of the pages published in *AER* in all three time periods (for the 1980s, this figure is as high as 85%). Over the past few decades, a clear dominance emerges of MIT followed by Harvard University, which together were responsible for around 30% of the pages published from 2004 to 2008 and from 1984 to 2008. In line with the results from table 1, MIT is again ranked before Harvard. In this top 10 list, LSE appears as the only non-American university (2004–2008 period)³⁶.

³⁶ Ann Rute Cardoso, Paulo Guimarães, and Klaus F. Zimmermann (2010: 623) provide a nice overview of studies that discuss the productivity gap between North-American and European universities. They also refer to studies that stress that the gap will decrease over time. Their results focusing on labor economics PhD graduates

TABLE 4: TOP INSTITUTIONS 1981–1990 AND 2001–2010

1981–1990		
Institutions	Number of Pages	Percent of Total Pages
Massachusetts Institute of Technology	539	4.7
Princeton University	510	4.5
Harvard University	500	4.4
University of Chicago	432	3.8
University of California, Los Angeles	333	2.9
University of Pennsylvania	312	2.7
University of Michigan	283	2.5
Stanford University	267	2.3
University of California, Berkeley	260	2.3
University of Western Ontario	228	2
Yale University	200	1.8
Total	3864	33.9
2001–2010		
Institutions	Number of Pages	Percent of Total Pages
Harvard University	1049	5.5
University of California, Berkeley	875	4.6
University of Chicago	758	4
Stanford University	749	3.9
Massachusetts Institute of Technology	651	3.4
Yale University	493	2.6
Princeton University	483	2.5
Columbia University	479	2.5
University of Pennsylvania	456	2.4
New York University	399	2.1
University of California, Los Angeles	382	2
Northwestern University	377	2
University of Michigan	343	1.8
London School of Economics	299	1.6
University of Maryland	267	1.4
University of British Columbia	253	1.3
University of California, San Diego	204	1.1
Carnegie Mellon University	201	1.1
Total	7899	45.9

Notes: Institutions that contributed 200 pages or more to *AER* (based on authors affiliation). Only primary journal articles are included (*Papers and Proceedings* are excluded).

in Europe or the US (year 2000-2005) indicate that European graduates publish on average more articles, but in journals of lower average quality. They also provide potential explanations for it: “This may be because, when hired by a top-USA institution, graduates already have several good acceptances or because of more focused research efforts and clearer career incentives. This could also be because the American-based institutions still provide a better academic environment to reach the standard leading journals, which are still closer attached to the USA. Another explanation is that the rising quantity and the increased visibility the European researchers have provided over recent years needs more time to breed higher quality research and lead to high quality research and lead to high quality journal publications. Part of this is related to the fact that the very many new European-based journals where Europeans publish more than proportionally need many more years to move up the quality ladder in international competition. Publication strategies also seem to differ across both continents. In the USA, PhD graduates aim at a decent number of articles in “A-journals” before the tenure decision; the probability of acceptance in those journals is low, and hence delays to acceptance and publication of papers are longer. In Europe, young researchers often publish before defending their PhD; journals are not necessarily A-ranked; and the rank of the journals (although of rising importance) is not yet so crucial for the tenure decision, which often follows other rules” (p. 635).

TABLE 5: TOP TEN INSTITUTIONS BASED ON CONTRIBUTORS' PH.D. INSTITUTION

1950-1959		
Institutions	Number of Contributors	Percent of Total Sample of 287 Contributors
Harvard University	62	21.6
Columbia University	38	13.24
University of Chicago	34	11.85
University of California	20	7
University of Wisconsin	13	4.53
Yale University	12	4.18
University of Pennsylvania	11	3.83
University Michigan	10	3.48
Cornell University	8	2.79
Stanford University	8	2.79
Total	216	75.29
1984-1988		
Institutions	Number of Contributors	Percent of Total Sample of 435 Contributors
Massachusetts Institute of Technology	70	16.1
Harvard University	55	12.6
University of Chicago	47	10.8
Stanford University	36	8.3
University of California, Berkeley	35	8
Princeton University	30	6.9
Yale University	27	6.2
University of California, Los Angeles	26	6
University of Pennsylvania	22	5.1
Northwestern University	20	4.6
Total	368	84.6
2004-2008		
Institutions	Number of Contributors	Percent of Total Sample of 600 Contributors
Massachusetts Institute of Technology	100	16.7
Harvard University	83	13.8
Stanford University	43	7.2
Princeton University	39	6.5
University of Chicago	32	5.3
University of California, Berkeley	29	4.8
University of Minnesota	25	4.2
Yale University	25	4.2
London School of Economics	24	4
Northwestern University	22	3.7
Total	422	70.4

Notes: Data for the years 1950 to 1959 are from Frank R. Cleary and Daniel J. Edwards (1960). For the other years, we include short papers (e.g., notes, comments, replies) but exclude *Papers and Proceedings* items.

Interestingly, Amir and Knauff (2008: 185) propose an alternative criterion on which to judge economics departments. Their method ranks the departments based not on research productivity but on the strength of the Ph.D. program as measured by a department's ability to place doctoral graduates in top-level economics departments or business schools: "Within the respective context, faculty hires probably constitute a more reliable and stable indicator of influence than journal citations." Their results also indicate a clear dominance by MIT (number one) and Harvard University (number two), followed by Stanford University, Princeton, the University of Chicago, Yale University, UC Berkeley, Oxford University, the University of Minnesota, Northwestern University, and the London School of Economics. Their results are very similar to our findings for recent years as presented in table 5.

2.2 Country Rankings

Rather than focusing only on institutions, we felt it might also be interesting to examine the performance of individual countries.³⁷ In terms of the number of publications per capita,³⁸ Israel is at the top of the list in both periods, followed by the U.S. This result is comparable to Martin Kocher and Matthias Sutter's (2001) finding for 15 economics journals from a period that includes 1977, 1982, 1987, 1992, and 1997. Looking at the entire period at once, they also found that Israel was number one and the U.S. number two. Hence, these results seem to indicate that U.S. dominance decreases once population size is controlled for. As we both are Swiss, we are pleased to see that Switzerland has improved its position from number six (1984–1988) to number three (2004–2008).

³⁷ For previous studies in that area, see, e.g., Geoffrey M. Hodgson and Harry Rothman (1999) or Martin Kocher and Matthias Sutter (2001).

³⁸ This approach assumes that academic resources devoted to economics are highly correlated with a country's population. Such may not, however, be the case for populous developing countries (Martin Kocher and Matthias Sutter 2001). This problem can thus be mitigated by looking at only the top 10 countries.

TABLE 6: TOP COUNTRIES PUBLISHING IN *AER*

Country	Period 1984–1988	Period 2004–2008	Rank 1984–1988	Rank 2004–2008
Israel	5.35	2.55	1	1
USA	3.17	2.37	2	2
Switzerland	0.61	1.33	6	3
Sweden	0.72	1.32	5	4
Norway	–	1.28	–	5
United Kingdom	–	1.00	–	6
Canada	2.10	0.95	3	7
Netherlands	–	0.79	–	8
Ireland	0.57	0.71	8	9
Belgium	0.41	0.57	10	10
Australia	0.75	–	4	–
New Zealand	0.61	–	7	–
Finland	0.41	–	9	–

Notes: Number of *AER* publications; top 10 countries in each period; per capita in millions.

2.3 Top Papers

After dealing with institutions and countries, the natural progression is to “zoom in” and focus on top papers and top authors published in *AER*. To identify the former, we adopt an imperfect but widely available proxy for *quality*, namely citations.³⁹ More specifically, we work with the 2009 *Journal Citation Reports*⁴⁰ and produce the results listed in table 7. The most cited papers to date are Armen Alchian and Harold Demsetz’s (1972) study “Production, Information Costs, and Economic Organization,” followed by Michael C. Jensen’s paper “Agency Costs of Free Cash Flow, Corporate Finance, and Takeovers.”⁴¹ Interestingly, Joseph Stiglitz appears three times and Carl Shapiro twice. To gain insight into an author’s perspective, we asked Harold Demsetz whether he would be willing to describe to us his personal experience with this paper, and are thankful that he generously agreed. Here is his detailed response to our request:⁴² “The paper began life while I was on the faculty of the University of Chicago Graduate School of Business and Alchian was in residence for a year as a visitor from UCLA. My prior position was on the faculty of the UCLA Economics

³⁹ We discuss the limitations of citations at a later stage.

⁴⁰ This dataset includes a reloading on September 22, 2010, of data originally collected in December 2010.

⁴¹ When Google Scholar is used as the search engine, these two papers are also ranked top among the papers reported in Table 7 (accessed January 25, 2011).

⁴² Email correspondence, January 2011.

Department. There, Armen and I got to know each other well. After departing UCLA for Chicago, I urged my colleagues to invite Alchian for a year's visit in the hope that we might be able to lure him from Los Angeles. Hence, we both were in Chicago. Toward the end of the year's visit (he decided not to remain at the U. of Chicago) we began to discuss the emptiness of the theory of the firm that was being taught. That is, the theory simply described the objective of the firm—to maximize profit—and said next to nothing—other than equating marginal cost to marginal revenue—about the problems and methods the firm faced and used. Our discussion gave rise to the notion of team productivity and to the difficulty this caused when attempting to ascertain the value of the marginal product of a worker's service. This difficulty led us to see the organization of the firm, its methods of compensation, and the monitoring of activities within the firm as responses to problems created by team-oriented production. The chief contribution of the paper, as I see it, was to break through the simplistic model that extended its reach to virtually all textbooks and most professional papers and to provide the profession with an eye-opening new approach to the theory of the firm. Our discussion of the shirking problem was transformed by Jensen and Meckling into an agency problem and became part of the larger problem of the financial structure of the firm. Our way of viewing the firm then permeated a variety of nooks and crannies in economics. We knew we had produced a good paper centered on a new way of viewing the general problem of cooperation between persons possessed of their own self-interests, but the great success of the paper came as somewhat of a surprise. The paper was actually written by way of a series of independently written drafts that we exchanged by way of mail (or were we using computers at that time?). We worked over a draft that was sent by the other co-author, sent it back with suggestions, revisions, and new ideas, and then went through the process again and again until, at last, we thought we had reached the final product. The paper, then, truly was a joint product produced by a team (!) that had somehow

resolved the team production problem that our paper centered on, though, to this day, I cannot say which of us wrote which part of the paper. We sent it off to the *AER*. Happily, they accepted it. Shortly thereafter, UCLA offered me a handsome arrangement to visit for a year; with much difficulty, and after too long a visit, I chose to remain at UCLA.”

The first issue of the *American Economic Review* in 2011, published to celebrate the *AER*’s centenary, is a report that identifies the top 20 articles in the journal’s history as judged by a committee composed of Kenneth J. Arrow, B. Douglas Bernheim, Martin S. Feldstein, Daniel L. McFadden, James M. Poterba, and Robert M. Solow (chair). In the first page of the report, the committee outlines its decision process: “We decided against trying to define formally the criteria for inclusion: they surely comprise sheer intellectual quality, influence on the ideas and practices of economists, and general significance or breath; but it would be fruitless to try to specify the marginal rates of substitution among these and other qualities. We were looking for 20 admirable and important articles.” As a starting point, the committee used citation counts and the number of searches in JSTOR: “This is obviously important and relevant information, but not decisive on its own. Citation counts are biased in favor of subfields of economics with the largest populations. There is also a bias in favor of moderately recent articles, if only because the number of potential readers and writers has been increasing in time; very recent articles suffer from the fact that citations build up over time. In any case we were expected to use our judgment about quality and significance. So we used the citation and JSTOR data only to give us a large group of eligible [articles].” They then point out that they were “worried especially about overlooking articles in the very early days of the *AER*, some by great names in the history of economics.” Their decision process thus unfolded as follows: “In the event, our early ballots showed an encouraging unanimity or near-unanimity, especially about the leading candidates. We very quickly converged on the Top 15 articles. There were occasional differences of opinion, only to be expected from a

group with diverse interests, as we filled in the remaining three to five places.” Their final list (Arrow et al. 2011: 2–8) is reproduced below in alphabetical order:

- Alchian, Armen A., and Harold Demsetz. 1972. “Production, Information Costs, and Economic Organization.” *American Economic Review*, 62(5): 777–95.
- Arrow, Kenneth J. 1963. “Uncertainty and the Welfare Economics of Medical Care.” *American Economic Review*, 53(5): 941–73.
- Cobb, Charles W., and Paul H. Douglas. 1928. “A Theory of Production.” *American Economic Review*, 18(1): 139–65.
- Deaton, Angus S., and John Muellbauer. 1980. “An Almost Ideal Demand System.” *American Economic Review*, 70(3): 312–26.
- Diamond, Peter A. 1965. “National Debt in a Neoclassical Growth Model.” *American Economic Review*, 55(5): 1126–50.
- Diamond, Peter A., and James A. Mirrlees. 1971. “Optimal Taxation and Public Production I: Production Efficiency.” *American Economic Review*, 61(1): 8–27.
- Diamond Peter A., and James A. Mirrlees. 1971. “Optimal Taxation and Public Production II: Tax Rules.” *American Economic Review*, 61(3): 261–78.
- Dixit, Avinash K., and Joseph E. Stiglitz. 1977. “Monopolistic Competition and Optimum Product Diversity.” *American Economic Review*, 67(3): 297–308.
- Friedman, Milton. 1968. “The Role of Monetary Policy.” *American Economic Review*, 58(1): 1–17.
- Grossman, Sanford J., and Joseph E. Stiglitz. 1980. “On the Impossibility of Informationally Efficient Markets.” *American Economic Review*, 70(3): 393–408.
- Harris, John R., and Michael P. Todaro. 1970. “Migration, Unemployment and Development: A Two-Sector Analysis.” *American Economic Review*, 60(1): 126–42.
- Hayek, Friedrich A. 1945. “The Use of Knowledge in Society.” *American Economic Review*, 35(4): 519–30.
- Jorgenson, Dale W. 1963. “Capital Theory and Investment Behavior.” *American Economic Review*, 53(2): 247–59.
- Krueger, Anne O. 1974. “The Political Economy of the Rent-Seeking Society.” *American Economic Review*, 64(3): 291–303.
- Krugman, Paul. 1980. “Scale Economies, Product Differentiation, and the Pattern of Trade.” *American Economic Review*, 70(5): 950–59.
- Kuznets, Simon. 1955. “Economic Growth and Income Inequality.” *American Economic Review*, 45(1): 1–28.
- Lucas, Robert E., Jr. 1973. “Some International Evidence on Output-Inflation Tradeoffs.” *American Economic Review*, 63(3): 326–34.

- Modigliani, Franco, and Merton H. Miller. 1953. “The Cost of Capital, Corporation and Finance and the Theory of Investment.” *American Economic Review*, 48(3): 261–97.
- Mundell, Robert A. 1961. “A Theory of Optimum Currency Areas.” *American Economic Review*, 51(4): 657–65.
- Ross, Stephen A. 1973. “The Economic Theory of Agency: A Principal’s Problem.” *American Economic Review*, 63(2): 134–39.
- Shiller, Robert J. 1981. “Do Stock Prices Move Too Much to Be Justified by Subsequent Changes in Dividends?” *American Economic Review*, 71(3): 421–36.

Four articles out of this list also appear in table 7; namely, the contributions of Armen A. Alchian and Harold Demsetz, Avinash K. Dixit and Joseph E. Stiglitz, Friedrich A. Hayek, and Milton Friedman.

TABLE 7: TOP 10 AER PAPERS BY CITATION

Title	Authors	Publication Date	Citations
PRODUCTION, INFORMATION COSTS, AND ECONOMIC ORGANIZATION	ALCHIAN, AA DEMSETZ, H	62(5) 1972	2215
*AGENCY COSTS OF FREE CASH FLOW, CORPORATE FINANCE, AND TAKEOVERS	JENSEN, MC	76(2) 1986	2014
CREDIT RATIONING IN MARKETS WITH IMPERFECT INFORMATION	STIGLITZ, JE WEISS, A	71(3) 1981	1462
MONOPOLISTIC COMPETITION AND OPTIMUM PRODUCT DIVERSITY	DIXIT, AK STIGLITZ, JE	67(3) 1977	1421
THE USE OF KNOWLEDGE IN SOCIETY	HAYEK, FA	35(4) 1945	1159
EQUILIBRIUM UNEMPLOYMENT AS A WORKER DISCIPLINE DEVICE	SHAPIRO, C STIGLITZ, JE	74(3) 1984	1053
A SENSITIVITY ANALYSIS OF CROSS-COUNTRY GROWTH REGRESSIONS	LEVINE, R RENELT, D	82(4) 1992	1050
ROLE OF MONETARY POLICY	FRIEDMAN, M	58(1) 1968	1000
NETWORK EXTERNALITIES, COMPETITION, AND COMPATIBILITY	KATZ, ML SHAPIRO, C	75(3) 1985	993
MIGRATION, UNEMPLOYMENT AND DEVELOPMENT —2-SECTOR ANALYSIS	HARRIS, JR TODARO, MP	60(1) 1970	974
*CLIO AND THE ECONOMICS OF QWERTY	DAVID, PA	75(2) 1985	974

Notes: *Articles in the *Papers and Proceedings* (entitled *AER Papers* until vol. 100, no. 4). Data based on the 2009 *Journal Citation Reports* including the reloading on September 22, 2010. Data accessed December 2010.

2.4 Publishing Frequency

In Table 8, using data that covers almost the entire 100-year history of the journal, we report the frequency with which authors publish in *AER*.⁴³ As the table shows, 67% of the authors publish only once, 16% twice, and 7% three times, indicating that only a minority (less than 10%) were able to publish more than three times in *AER*. Figure 1 then graphs what we would call a “citation Lorenz curve,” thereby providing a citation inequality proxy for all articles published in *AER* between 1911 and 2010. This figure reveals a significant level of citation inequality⁴⁴ (Gini coeff. = 0.75); for example, 20% of the articles are responsible for 80% of the citations. Interestingly, the sports environment, a highly competitive arena in which income rather than citations is a dominant force, reports lower “income inequality” values.⁴⁵

TABLE 8: DISTRIBUTION OF PUBLICATIONS AMONG AUTHORS (1911–2010)

NUMBER OF PUBLICATIONS	FREQUENCY	PERCENT
1	4288	66.98
2	1046	16.34
3	464	7.25
4	214	3.34
5	140	2.19
6	86	1.34
7	44	0.69
8	35	0.55
9	22	0.34
10	21	0.33
11	14	0.22
>11	28	0.45
Total	6402	100

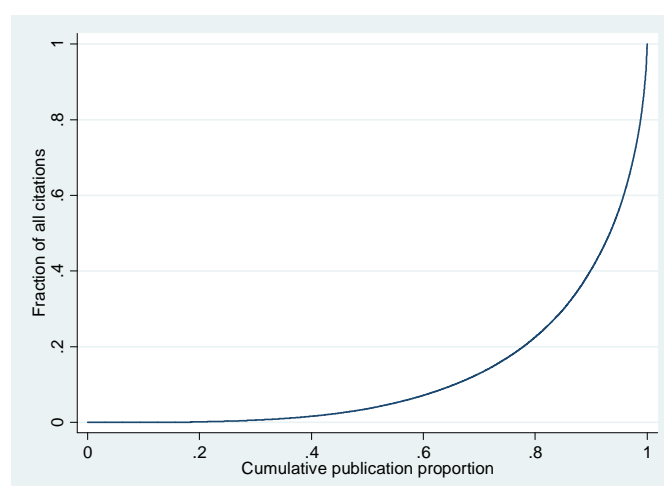
Notes: Measured up until vol. 100, no. 4; these numbers include *Papers and Proceedings*.

⁴³ Until vol. 100, no. 4.

⁴⁴ Samuelson (2004: 60) recalls the following exchange: “Once I asked my friend the statistician Harold Freeman, ‘Harold, if the Devil came to you with the bargain that, in exchange for your immortal soul, he’d give you a brilliant theorem, would you do it?’ ‘No,’ he replied, ‘but I would for an inequality.’”

⁴⁵ Rodney D. Fort (2003: 203) reports Gini coefficients for a tournament outcome in men’s and women’s pro golf of 0.635 (men) and 0.621 (women) but argues that in team sports, the values are lower (up to 0.5).

FIGURE 1: LORENZ CURVE OF CITATIONS 1911–2010



2.5 Top Authors

Because of the strong “superstar effect” illustrated in figure 1, it should be interesting to identify who these authors are who have published 12 or more contributions in *AER* (including *Papers and Proceedings*) throughout almost its entire 100-year history.⁴⁶ The results, presented in table 9, demonstrate that out of these 27 researchers, 8 are Nobel Laureates and the 3 who are not Nobel Laureates (Martin S. Feldstein, Kenneth E. Boulding, and Lawrence H. Summers) have all been awarded the John Bates Clark Medal. When we include *Papers and Proceedings*, Joseph E. Stiglitz has the most publications followed by William J. Baumol (Stiglitz: 36; Baumol: 29), a position they maintain when *Papers and Proceedings* are excluded (Stiglitz: 24; Baumol: 22). One column also reports the total accumulated citations of the 27 authors’ *AER* papers, and again Stiglitz and Baumol lead the group. Stiglitz is also the top researcher with the highest average citations per article, followed by George J. Stigler (when *Papers and Proceedings* are included) or Milton Friedman (when they are not). Table 9 also reports “dry holes”; that is, publications that so far have received no citations (Laband and Tollison 2003). Around two-thirds of these top

⁴⁶ Data up until vol. 99, no. 5.

authors have one or several *AER* dry holes when *Papers and Proceedings* are included, a number that increases to over 50% when they are not. Thus, not even the top researchers whose contributions have so enlightened our knowledge in economics can escape dry holes. For instance, the 2003 study by David N. Laband and Robert D. Tollison, which focuses on 73 and 91 economics journals for the years 1974 and 1996, respectively, shows an average of 26% dry holes. However, as Thomas Mayer (2004: 624) points out, not all dry holes are useless: some can terminate a particular research program or may settle or solve a puzzle in such a way that the papers are not cited. Thus, a dry hole may actually have an undetectable surface that marks a point of no return.

In addition, Frey (forthcoming: 3) makes the point that “works are often cited because they are considered to be wrong and not because they are taken to be a valuable contribution to knowledge.”⁴⁷ Thus, citations can be a sign that subsequent authors are generalizing a model or applying the knowledge or technique to another problem. However, there are also so-called “hat-tipping” citations, made to please authors that could be potential referees, demonstrate that the relevant literature has been read, or even in the hope that cited authors will reciprocate (Mayer 2004: 624). Thus, dry holes should be seen not as a waste but rather as “veritable proof of healthy competition” (Hendrik P. van Dalen and Arjo Klammer 2005: 412). Overall, as George J. Stigler, Stephen M. Stigler, and Claire Friedland (1995: 344) point out, the network of citations is the “product of a complex combination of factors, ranking from scientific influence and social contact to an element of pure chance in the timing of publication of accepted papers.”

⁴⁷ For a detailed discussion of the advantages and shortcomings of citations, see Osterloh and Frey (2009).

TABLE 9: TOP “SUPERSTARS” IN *AER* (12 AND MORE PUBLICATIONS)

Author	NAER Publ.	Nobel Prize	John Bates Clark Medal	Total Citations Accumulated	AVG Citations per Article	Dry Holes	NAER Publ.*	Total Citations Accumulated*	AVG Citations per Article*	Dry Holes*
STIGLITZ, JE	36	YES	YES	6963	193.4	0	24	6161	256.7	0
BAUMOL, WJ	29	NO	NO	2155	74.3	1	22	1945	88.4	1
CLARK, JM	24	NO	NO	139	5.8	7	10	112	11.2	3
EISNER, R	20	NO	NO	271	13.6	2	13	199	15.3	2
BUCHANAN, JM	19	YES	NO	730	38.4	2	13	635	48.8	2
SMITH, VL	19	YES	NO	1383	72.8	0	15	1067	71.1	0
FELDSTEIN, M	18	NO	YES	573	31.8	2	9	469	52.1	0
BOULDING, KE	15	NO	YES	118	7.9	2	4	20	5.0	1
MODIGLIANI, F	15	YES	NO	1140	76.0	0	12	938	78.2	0
FISHER, I	14	NO	NO	35	2.5	6	8	31	3.9	1
FRIEDMAN, M	14	YES	YES	1288	92.0	3	9	1217	135.2	1
HELPMAN, E	14	NO	NO	1273	90.9	0	14	1273	90.9	0
NELSON, RR	14	NO	NO	813	58.1	2	6	281	46.8	1
ROTH, AE	14	NO	NO	1098	78.4	0	14	1098	78.4	0
STIGLER, GJ	14	NO	NO	1487	106.2	0	9	1151	127.9	0
HANSEN, AH	13	NO	NO	119	9.2	6	7	114	16.3	3
REYNOLDS, LG	13	NO	NO	83	6.4	3	6	73	12.2	0
SAMUELSON, PA	13	YES	YES	891	68.5	1	10	837	83.7	1
AKERLOF, GA	12	YES	NO	857	71.4	2	7	649	92.7	0
BACH, GL	12	NO	NO	78	6.5	5	10	47	4.7	4
BLINDER, AS	12	NO	NO	1023	85.3	0	5	665	133.0	0
FETTER, FA	12	NO	NO	16	1.3	3	9	12	1.3	2
HART, AG	12	NO	NO	10	0.8	7	6	6	1.0	3
SMITHIES, A	12	NO	NO	26	2.2	3	5	12	2.4	1
STEIN, JL	12	NO	NO	318	26.5	2	11	307	27.9	2
SUMMERS, LH	12	NO	YES	862	71.8	0	6	511	85.2	0
TOBIN, J	12	YES	YES	781	65.1	0	4	299	74.8	0

Notes: *AER Papers and Proceedings are excluded, as is R. G. Blakey (mainly Revenue Acts).

Citation can also be driven by fashion even though fashionable topics may not be the most promising avenues for scientific progress (Hendrik P. van Dalen and Arjo Klammer, 2005). In fact, fashion in its compact mass may *deform* our scientific “knowledge spacetime.” Fortunately, however, through publication elsewhere in the journal universe or as human knowledge on the World Wide Web, dry holes can still emit “radiation” and retain a finite chance of being cited in the future. Nonetheless, this chance might be inversely proportional to the mass of dry holes, making this radiation difficult to see. Yet dry holes also provide an opportunity to create new fashion. The challenge is to find the right dry hole, which is itself a difficult task because of their almost invisible interior. Admittedly, relatively new earthbound search technologies like *EconLit*, *Journal Citation Reports*, or the Internet can help researchers track down dry holes. In fact, if one *assumes* that economics is the “Queen of the Social Sciences,”⁴⁸ one might even find a supermassive dry hole at the center of our discipline.

3. Cooperation, Coauthorship, and Alphabetical Name Ordering

Having found such a superstar market and seeing that so many authors are able to publish only once in *AER*, one might wonder just how much cooperation there is in academia? According to Bruno S. Frey (forthcoming: 3), “modern scientific activity is based on a marked division of labor. The scientific production process has increasingly been divided into neatly separated steps. For instance, in economics, it has become customary that

⁴⁸ In a paper published in *De Economist*, the Quarterly Review of the Royal Netherlands Economic Association, Bruno S. Frey (2006a: 295) points out in his introduction that economics “is often taken to be the ‘Queen of the Social Sciences’”. There is a higher degree of consensus about its approach and content than in many other social sciences (such as, for instance, in sociology or psychology). Many economists jump to the conclusion that they do have considerable influence on society. They believe that economics makes a major and beneficial contribution to the solution of economic and social problems. They routinely refer to the many economists called as policy advisors, and the prominent positions some economists have attained in politics. But such activities refer to the influence economics may have on the *input* side. Whether economics as a discipline does indeed have an *effect* on the economy refers to the *outcome* or *output* side. Economists’ input may or may not have an impact; and it may raise or lower social welfare. The widespread belief among economists of the large effect of economics on society is (at least so far) not based on any convincing empirical evidence.”

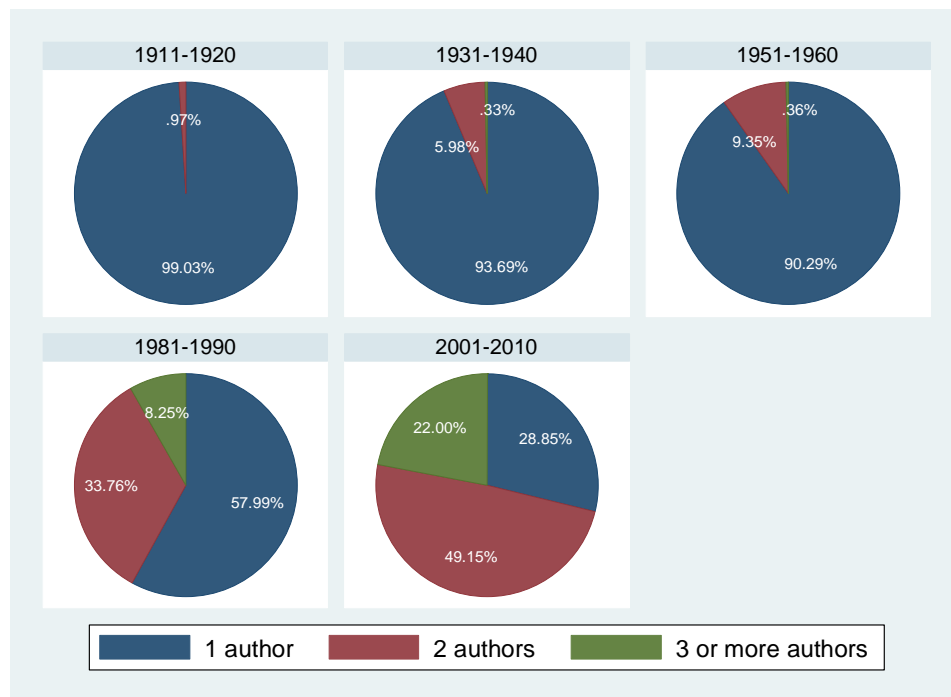
a young scholar, usually a graduate student, collects the data, a second scholar undertakes the econometric analysis, and a third scholar interprets the results and writes the paper. As a consequence, single authorship has become an exception, and an increasing number of papers have three or even four authors.” Besides specialization, other factors may also explain such tendencies as a “greater pressure to publish, and even a decrease in the willingness of economists to assist each other without receiving authorship credit” (Aidan Hollis 2001: 504).

Figure 2 uses pie charts to show the development of cooperation over time in authoring *AER* papers. In line with Frey’s observations, we find that single-authored contributions have significantly decreased over time. Between 1911 and 1920, 99% of the papers were single-authored, a number that had decreased to only 28% by the 2001 to 2010 period. Hence, whereas in the 1980s, over 50% of the papers were single authored, the picture has changed drastically over the past 10 years. Currently, close to 50% of contributions are the result of cooperation between two authors, and the number of contributions listing two or three (or even more) authors has increased. During the 1960s, in contrast, less than 1% of the papers were authored by three or more authors, but by the 2001 to 2010 period, this figure had increased to 22%. Frey (forthcoming: 3) criticizes this situation on the grounds that, although each author “formally acknowledges full responsibility for the content of the paper. However, realistically none of the individual authors can confidently judge whether the other authors have done their work carefully and sincerely. (...) Each participant in a particular scientific endeavour has to trust that the others do their work carefully. It is generally assumed that reliance on trust is well taken, but there is certainly no guarantee, especially when all the authors are under strong publication pressure.” He also points out that “[t]he division of labor has led to a more efficient and rapid output of scientific results but favors partial views and discourages comprehensive considerations” (2). Such a situation is substantially different

from that described by Harold Demsetz in his collaboration with Armen Alchian (i.e., a truly joint product about which, to this day, the authors cannot say who wrote which part).

Are our results similar to those obtained by other studies? Hollis's (2001) analysis of data on 5,277 journal publications by 339 academic economists (a total of 6,656 author-years) does point to some similarity. His study, which is based on individuals who graduated from U.S. and Canadian economics departments from 1965 to 1981 and covers publications until 1997, identifies 42.52% single-author contributions, 44.51% two-author papers, and a remaining 12.97% that were collaborations between three authors or more. These results fall somewhere in between the outcomes for *AER* papers demonstrated in our final two pie charts (1981–1990 and 2001–2010).⁴⁹

FIGURE 2: COOPERATION IN *AER* OVER TIME



Notes: *AER Papers and Proceedings* not included.

⁴⁹ Our focus on *AER* publications precludes the exploration of life cycle relationships; however, Aidan Hollis (2001) finds that the average level of coauthorship (arithmetic mean of the number of authors for all papers published by an individual during the period) increases with the years since graduation (from 1.4 to 1.9).

In an environment in which cooperation is increasing, it might be asked whether the process of deciding the order of authors could raise issues. In a study of 1,278 multiauthored articles from 11 mainstream economics journals between 1997 and 1999, C. Mirjam van Praag and Bernard M. S. van Praag (2008) find not only that 88% of multiauthored papers have alphabetical name ordering but that such ordering matters. For example, being an A-author who is often listed first is beneficial for a researcher's reputation and academic performance. Thus, even in an environment in which alphabetical ordering is so widespread, problems can still arise because this practice apparently disadvantages authors at the end of the alphabet. On the other hand, a nonalphabetical order may signal "uneven contributions, differences in hierarchical positions or the relative academic reputations of co-authors" (782–783). Thus, although van Praag and van Praag (2008: 783) define nonalphabetical ordering as "merit-based," they emphasize that the outcome of such merit-based ordering may also be alphabetical. Besides focusing on productivity, they also "estimate the determinants of deviating from the alphabetical name ordering strategy" and describe such a goal as "novel." They further point out that individual "contributions to co-authorship are unobserved. Truthful statements on this delicate matter are difficult to collect. Therefore we assume that the unequal distribution of contributions is a function of relevant observable aspects of inequality between authors of a specific article" (789). They therefore suggest possible observable dimensions, such as inequality in scientific weight (number of publications in refereed journals), scientific age (year 2002 minus the year of first publication in a journal included in the SSCI), or affiliation. Their results indicate that the average scientific age and weight affect negative deviation from an alphabetical name order: more experienced groups of authors with more publications are less inclined to deviate from the alphabetical name order convention (791). Moreover, XY(Z)-authors use a nonalphabetical name order more often than ABC-authors. Interestingly, however, a journal's impact factor has no effect on the

use of alphabetical ordering. On the other hand, they also show that longer articles, which have a more powerful effect on an author's career, increase the probability of alphabetical ordering; for example, an article 10 pages longer than the average has a 3% higher probability of alphabetical name ordering (791-792). The number of authors, on the other hand, has a negative effect on the probability of alphabetical name ordering (791).

For this current analysis, we work with a database of *AER* articles published in 1984–1988 and 2004–2008 that excludes *AER Papers and Proceedings* but includes short papers like notes, comments, or replies (for a total of 614 *AER* articles published by two or more authors). We collect the background information on the authors via the Web, based mostly on available CVs. In line with van Praag and van Praag (2008), our results indicate a significant level of alphabetical name ordering (90.6%). To estimate the determinants of such ordering, we use a probit model in which the dependent variable is 1 if alphabetical name ordering occurs and 0 otherwise. The independent variables are group characteristics such as the share of female authors or the average academic age (year of publication – year Ph.D. obtained).⁵⁰ We also explore the institutional ranking difference (calculated as the difference between the best ranked author and the lowest ranked author within the group)⁵¹ and control for article length and the level of techniques used (number of equations, tables, and figures⁵²).

Table 10 presents the results, whose robustness we check by adding certain factors sequentially into the first specification. These findings indicate a negative relationship between academic age difference and the probability of alphabetical name ordering. On the other hand, rank difference and rank average are not statistically significant in most specifications. To assess the relationship between the number of coauthors and the

⁵⁰ The value is always positive as we calculate the difference between, academically speaking, the oldest and the youngest author of an article.

⁵¹ We used the previously discussed institutional rankings from Rabah Amir and Malgorzata Knauff (2008) that provide information on the best 58 institutions worldwide. Since our analysis includes universities that do not appear in their top 58 list, we allocate these institutions the position of 59.

⁵² We consider an equation to be a mathematical formula if it is either numbered in the article or is clearly separated from the text (placed by itself on a line).

probability of alphabetical name ordering, we use dummy variables rather than the number of authors used by Praag and van Praag (2008). We find a strong relationship between the number of coauthors and the probability of alphabetical name ordering: the values are around 8% lower for groups of three (two coauthors) than for groups of two (one coauthor). This effect is substantially stronger for groups of four or more: a 30% lower probability of alphabetical name ordering. What, then, happens in the cases of nonalphabetical name ordering? Apparently, it is merit that matters because in our data set higher ranked authors or authors with more experience do not dominate author order.

These two findings seem to indicate that authors have a problem with visibility. Indeed, as van Praag and van Praag (2008: 782) point out, “citations within articles, which clearly contribute to the reputation and visibility of the author cited, are shortened to ‘[first author] et al.’ as soon as there are more than two authors. Visibility is also constrained for other than first authors in frequently used search engines such as *EconLit*, which merely names the first author for articles by more than three individuals.”

TABLE 10: DETERMINANTS OF ALPHABETICAL NAME ORDERING IN *AER*
(1984–1988 AND 2004–2008)

Dependent Variable: Name Ordering (alphabetical = 1; nonalphabetical = 0)				
Independent Variables	[1]	[2]	[3]	[4]
Rank Difference	0.001 (0.19) <i>0.0001</i>	-0.003 (-0.68) <i>-0.0004</i>	-0.002 (-0.63) <i>-0.0003</i>	-0.005 (-1.13) <i>-0.001</i>
Rank Average	-0.007* (-1.76) <i>-0.006</i>	-0.006 (-1.45) <i>-0.001</i>	-0.005 (-1.27) <i>-0.001</i>	-0.004 (-0.85) <i>0.0004</i>
Academic Age Difference	-0.026*** (-3.43) <i>-0.018</i>	-0.018** (-2.18) <i>-0.002</i>	-0.002** (-2.10) <i>-0.002</i>	-0.022** (-2.38) <i>-0.003</i>
Female Share	-0.483 (-1.22) <i>-0.07</i>	-0.527 (-1.27) <i>-0.071</i>	-0.431 (-1.03) <i>-0.057</i>	-0.338 (-0.74) <i>-0.039</i>
2004–2008 Dummy	0.673*** (4.17) <i>0.107</i>	0.682*** (3.64) <i>0.101</i>	0.698*** (3.67) <i>0.102</i>	0.656*** (3.17) <i>0.083</i>
Two Coauthors		-0.504*** (-2.85) <i>-0.083</i>	-0.484*** (-2.72) <i>-0.078</i>	-0.532*** (-2.77) <i>-0.078</i>
Three or More Coauthors		-1.277*** (-3.89) <i>-0.34</i>	-1.198*** (-3.58) <i>-0.308</i>	-1.296*** (-3.49) <i>-0.323</i>
Length of Article		0.005 (0.48) <i>0.001</i>	0.0002 (0.01) <i>0.000</i>	0.004 (0.17) <i>-0.0004</i>
Total Equations			0.006 (1.07) <i>0.001</i>	0.001 (0.13) <i>0.0000</i>
Total Tables			-0.006 (-0.18) <i>-0.001</i>	-0.01 (-0.25) <i>-0.001</i>
Total Figures			-0.002 (-0.07) <i>-0.0003</i>	-0.011 (-0.31) <i>-0.001</i>
<i>N</i>	614	614	614	584
Prob.>chi ²	0.000	0.000	0.000	0.000
Pseudo <i>R</i> ²	0.077	0.127	0.132	0.192
JEL Code Variables	No	No	No	Yes

Notes: The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. Short papers (e.g., notes, comments, replies) are included. Coefficients in bold, *z*-statistics in parentheses, marginal effects (of an “average individual”) in italics (calculated at the point of the sample means in the case of a continuous variable). Since we include many universities that do not appear in the top 58 of the ranking developed by Amir and Knauff (2008), we allocate these universities a constant value of 59. The higher the ranking values, the lower the performance.

4. Demographic Characteristics of AER Authors

Having briefly explored the superstars in the *AER* publication process and the level of cooperation at the group level, we now analyze the demographic characteristics of *AER* authors with a focus on two obvious elements: experience (i.e., academic age as previously defined) and gender.

4.1 Academic Age

Arthur M. Diamond, Jr. (1986: 521), referring to Yoram Weiss and Lee L. Lillard (1982), points out that research articles can be interpreted as an investment in human capital, specifically as “investment in knowledge at the frontiers of the discipline. Such human capital, especially at major research graduate universities, increases a scientist’s productivity in the activities directly demanded by the university such as teaching, fund-raising, administration, and the gate-keeping activity of peer review. Life-cycle human capital investment models imply that as a scientist ages, his investment in human capital will decline for two reasons. One is that fewer periods remain in which additional human capital will be available. The other is that as a scientist ages his stock of human capital increases, thereby increasing the productivity of his time in nonresearch activity. (...) Related to the implication of life-cycle models that investment in human capital will decline with age is the implication that for a period of time the stock of human capital will be increasing even though the annual flow of investment in human capital is declining. This would occur so long as the annual investment remains greater than the annual loss of human capital due to depreciation.”

Focusing on the impact of age on the quantity and quality of output for Berkeley mathematicians, Diamond (1986) observes that both the quantity and quality of output (measured as articles per year, citations per article, pages per year, and citations per page) decline monotonically with age. An earlier study by Stephen Cole (1979) explores the

relationship between age and productivity in six fields (chemistry, geology, mathematics, physics, psychology, and sociology) using cross-sectional data from 1965 to 1969. Cole (1979) finds that age tends to have a slightly curvilinear effect on productivity, a result that is consistent across all six disciplines. That is, at the beginning of a career, a researcher's productivity rates increase, peaking in the late thirties or forties and then dropping off. Cole (1979: 964) also collected longitudinal data for mathematicians, choosing this group "because youth is believed to be more significantly correlated with creative productivity in math than in other science." Focusing on a cohort of mathematicians who received their doctorates between 1947 and 1950, he finds that the mean number of published papers barely changes between 1950 and 1974 (mean = 2.3–2.8). He also differentiates between nonproductive, weak, and strong publishers, observing that the proportion of the cohort who would be classified as active publishers remains quite constant, only varying between 17% (1950–54) and 14% (1970–1974). His results indicate that those who are productive at one time are the most likely to be productive at any other time. In addition, less than 10% of the cohort make a significant change in their productivity patterns during the course of their career. That is, highly productive mathematicians remain so as they grow older, while those who are not productive early in their careers rarely become more productive.

To measure the change in productivity over the life cycle, Sharon M. Oster and Daniel S. Hamermesh (1998) focus on 208 economists at 17 top research institutions. Their results indicate that between years 9 to 10 and 14 to 15 after Ph.D. completion, these economists (as a group) lose 29 to 32% of their output, and between years 9 to 10 and 19 to 20, they lose 54 to 60%. In other words, on average, the productivity loss is around 5% per year from the time of peak productivity. The median age of authors of full-length refereed articles in *AER*, the *Journal of Political Economy*, and *Quarterly Journal of Economics* was 36 in both the 1980s and the 1990s, and researchers under 36 were responsible for a large share of the publications

over four decades: 51.3% (1963), 61.4% (1973), 46.3% (1983), and 46.6% (1993). According to Hollis (2001), there is a rapid increase in total output at two years after graduation, which peaks a little bit higher in the fourth year and then begins to decline steadily (fewer publications and lower quality).

Taking our cue from these previous studies, we suspect that it may be informative to take a closer look at authors in the *AER* publications. We therefore collected data on the 1,818 authors of papers published during the years 1984–1988 and 2004–2008. The results for output based on academic age (year of publication – year Ph.D. obtained) are plotted in figures 3 and 4. In line with previous studies, we find that the highest level of creativity and output in economics is evident in younger scholars.⁵³ Moreover, in terms of differences

⁵³ This finding implies that young economists should not be discouraged from generating their own new ideas. In a discussion of age and human capital in his contribution to *Lives of the Laureates*, Gary S. Becker (2004: 258) exemplifies this principle: “I was surprised that the main hostility toward my work, at least as it was explicitly stated, came from economists, not non-economists. I began to realize that my original view was naïve. All disciplines have a strong and probably justified degree of intellectual conservatism. You do not give up ideas and concepts you have held for a long time without a fight. It is necessary to fight to get new ideas accepted.” Later he writes, “In 1959, I made the first public presentation of some of my results at a session of the annual meetings of the American Economic Association. I presented a short paper that compared rates of return to schooling and returns on physical capital in the United States. And the discussants, to my amazement, were absolutely outraged. Once again, I continued to be surprised by what I should have anticipated. What was it that so outraged my discussants? In retrospect it seems silly. They were outraged that I was treating education as an economic activity, believing that this assumption somehow denigrated the cultural or non-economic aspects of education. It was one of the more heated sessions of the meeting. I was taken aback, but truth be told, I did not lose any confidence about what I was doing because their comments seemed so silly to me. I could not really believe that senior economists—I was 29 years old at the time—were making such dumb comments on my paper” (260–261). The paper to which he was referring has been published in the *AER Papers and Proceedings* under the title “Underinvestment in College Education?” (1960, vol. 50, pp. 346–354). Such extreme reactions may seem surprising to today’s reader. Becker summarizes his findings as follows: “Several aspects of college education in the United States were examined in terms of their contribution to economic and military progress. The limited available evidence did not reveal any significant discrepancy between the direct returns to college education and business capital, and thus direct returns alone do not seem to justify increased college expenditures. This puts the burden on external or indirect returns since they would have to be important to justify increased expenditures. Unfortunately, very little is known about them; so a firm judgment about the extent of underinvestment in college education is not possible. Many recent discussions have emphasized the external contributions of scientists to economic and military progress and have called for large increases in scientific personnel. Such an increase could be accomplished with a small increase in total college expenditures. A large increase in expenditures would be warranted only if external returns were produced by a much larger fraction of all college graduates. A sizeable fraction of all persons with high I.Q.’s or grades do not go to college after graduating from high school. It appears that an increase in the fraction of able persons going to college would raise the average return from college. An improvement in the quality of college students may well be an effective way to raise the contribution of college education to progress” (354). Becker then “continued working on the economics of human capital and in 1962 published an article on it. It was in fact well received” (Becker 2004: 261). The paper, “Investment in Human Capital: A Theoretical Analysis,” was published in *Journal of Political Economy* (Becker 1962). Becker outlines subsequent events: “Then, in 1964, I published a book called *Human Capital: A Theoretical and Empirical Analysis: A Theoretical and Empirical Analysis, with Special*

between the 1980s and the 2000s, author age distribution seems to have shifted leftward. This decrease in age is statistically significant ($z = -5.38$). In figure 5, we then explore the publication performance of the *AER* superstars listed in table 9. Interestingly, these superstars' *AER* publication performance peaks substantially later (20 years post doctorate), indicating that publishing productivity for such top researchers peaks at a fairly late age.⁵⁴ It may be that their high research productivity helps reduce the allocation of their time away from research or it may simply be a sign of unwillingness: as Samuelson (2004: 59) admits, "I am notorious for shirking tasks I hate to do. I minimize administrative duties, displaying an incompetence in their performance that chokes off additional assignments."

Reference to Education. The long subtitle is now forgotten—it is now called *Human Capital*. Actually, I debated a long time before I used the title *Human Capital* because I had been aware that people said that if you call it 'capital' you are treating human beings as if they had no soul. Some people would make fun of it and call it 'human cattle,' suggesting that one is not treating humans as individuals. I knew that, and could have weaseled a little and called it 'human resources,' a phrase that was becoming common at the time. I decided to take the bull by the horns and title the book *Human Capital*, although it had this long subtitle to protect myself a little. By the time I finished this research, I was indeed convinced that human capital was a crucial concept to understand economic and social issues in many areas of life. Still, and this I will also confess, I was not prepared for the magnitude of its impact. Eventually, it would be referred to endlessly, and by that language—human capital—not only in academic writing but by politicians of both parties, journalists, even in ecclesiastical encyclicals. After a while some of the people who had resisted using this term began to think 'Well, look, if we call everything human capital and say we are investing in people, this can provide a good rationale for obtaining public monies'" (261). Today, a large set of journals include the notion of human resources in their title (e.g., the *Journal of Human Resources*, *Asia Pacific Journal of Human Resources*, *Journal of Human Resource Costing and Accounting*, and *International Journal of Human Resources Development and Management*). There are also journals with human capital in their title, including, for example, the *Journal of Human Capital*, a quarterly journal founded in 2007 and published by the University of Chicago. Using "human capital" as a Goggle search term produces about 11,300,000 results (January 21, 2010). Paul A. Samuelson (2004: 61) also points out that there "was never a time when I didn't strive to please myself. There have been those who thought that my fooling around with thermodynamics was an attempt to inflate the scientific validity of economics; even perhaps to snow the hoi polloi of economists who naturally can't judge intricacies of physics. Actually, such methodological excursions, if anything, put a tax on reputation rather than enhancing it. So what? Taxes are the price we pay for civilization. Such work is fun. And I perceive it adds to the depth and breadth of human knowledge."

⁵⁴ At the end of his contribution in his section Indian Summer, Samuelson (2004: 63) writes, "As I veer toward the traditional three score and ten, how do I feel about it? Goethe, who like Wagner and Verdi had a great long run, wrote that the difference between age and youth was that in youth, when you called on it, it was always there in response. By contrast, only on the best good days could the octogenarian attain the peak performance. To myself, I am sixty-nine going on twenty-five. All the days seem as good as ever. But, as the lyricist says and reason insists, the stock of what's left of the good times must shrink as you reach September."

FIGURE 3: PUBLISHING IN *AER* AND ACADEMIC AGE 1984–1988 ($N = 865$)

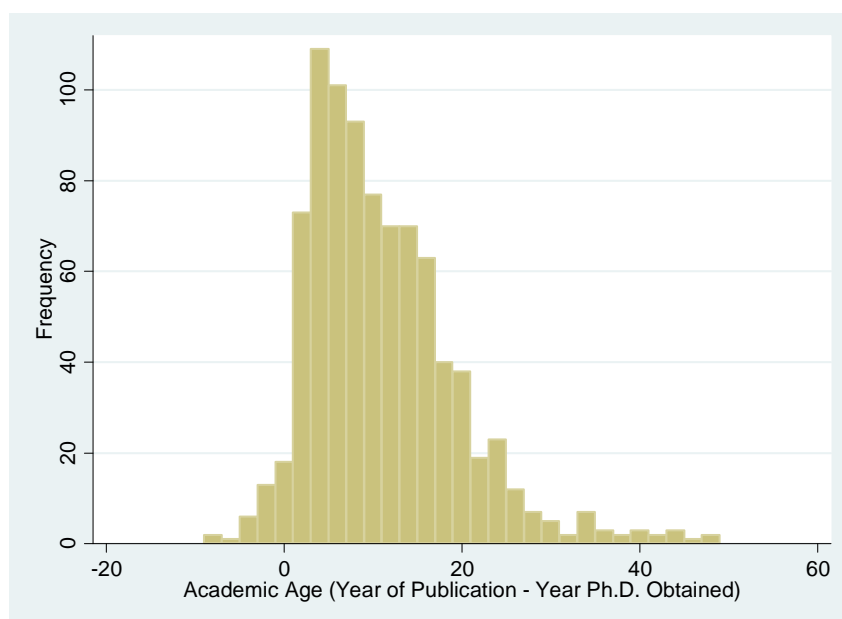


FIGURE 4: PUBLISHING IN *AER* AND ACADEMIC AGE 2004–2008 ($N = 953$)

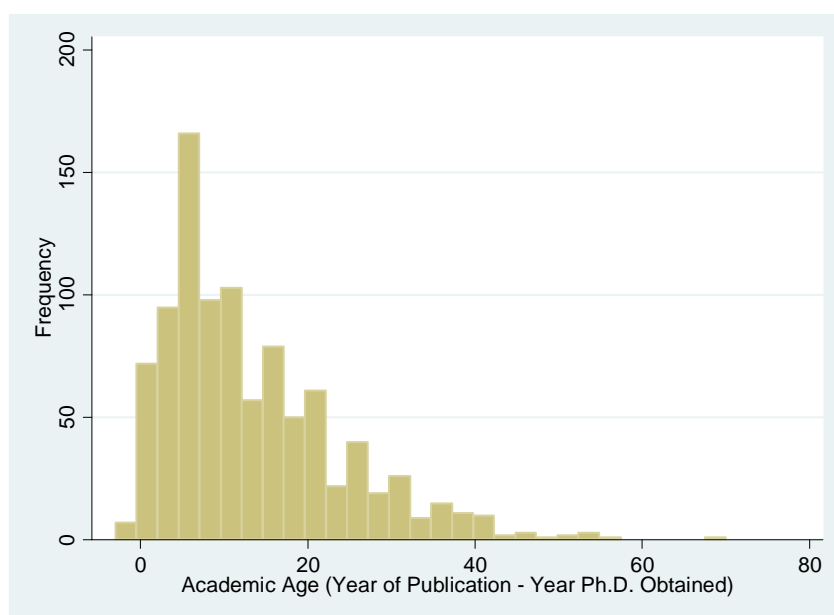
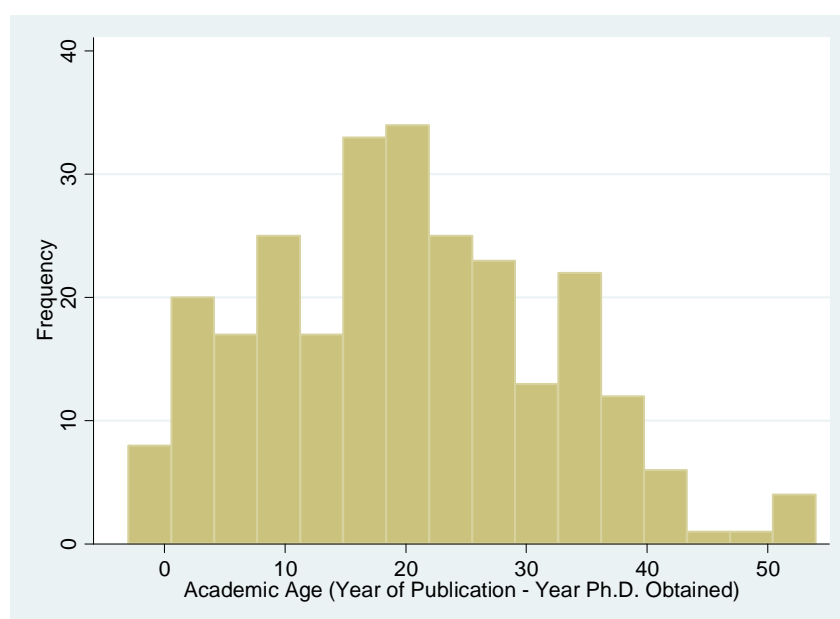


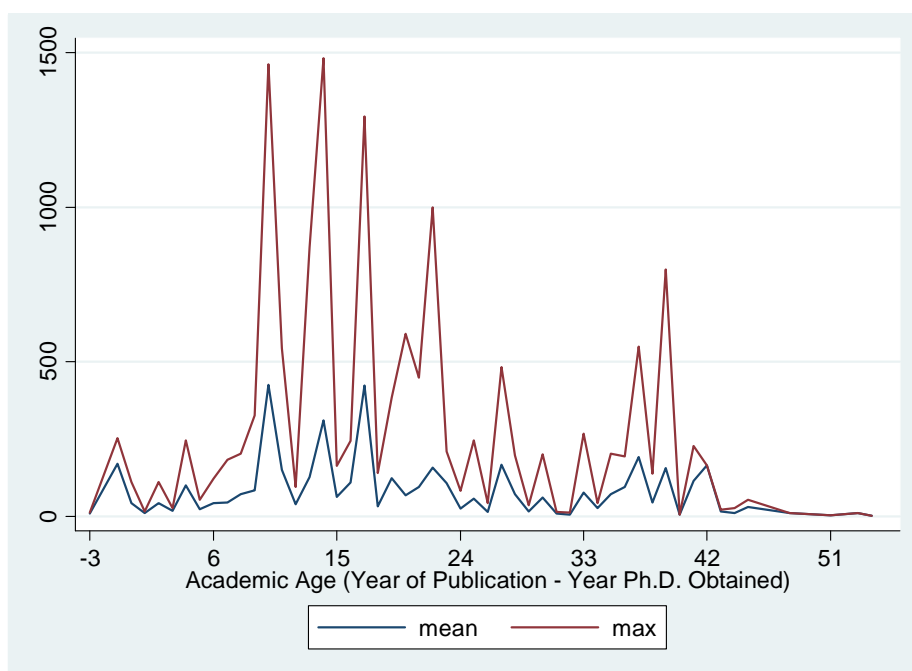
FIGURE 5: SUPERSTARS AND ACADEMIC AGE ($N = 261$)



Notes: Includes all researchers listed in table 9 except Boulding, who is not reported to have a Ph.D.; *Papers and Proceedings* excluded.

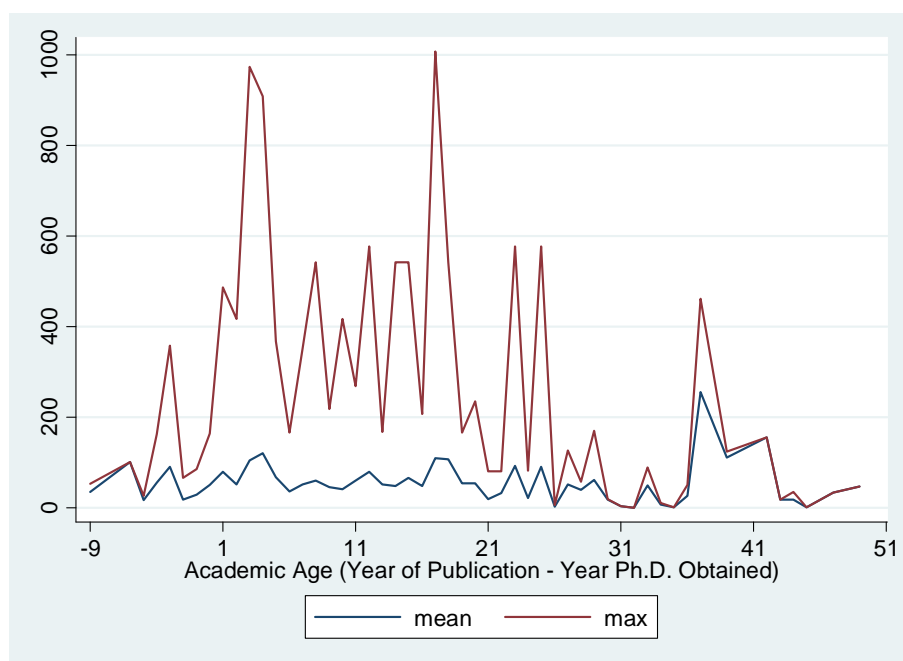
In figures 6 and 7, we focus on citations rather than publications. Specifically, figure 6 reports the citations of *AER* papers from the superstar list reported in table 9, while figure 7 focuses on *AER* articles published between 1984 and 1988 (excluding *Papers and Proceedings*). We measure the number of citations using the 2009 *Journal Citation Reports* (see earlier discussion), but to give the articles enough time to attract citations, we exclude data from 2004 to 2008. Because single papers drive the results, we observe many spikes in both figures, a finding that is not surprising given the high level of citation inequality reported in figure 1. Authors can even generate upward spikes 35 years after earning their doctorates. For superstars, many high spikes occur at an academic age somewhere between year 9 and year 23, whereas the peaks start emerging earlier among all the *AER* authors that published between 1984 and 1988. For these latter, the yearly mean of citations barely exceeds 200, while *AER* superstars generate higher mean values.

FIGURE 6: CITATIONS FOR *AER* SUPERSTARS



Notes: Number of observations = 411, *AER Paper and Proceedings* included.

FIGURE 7: CITATIONS FOR *AER* AUTHORS

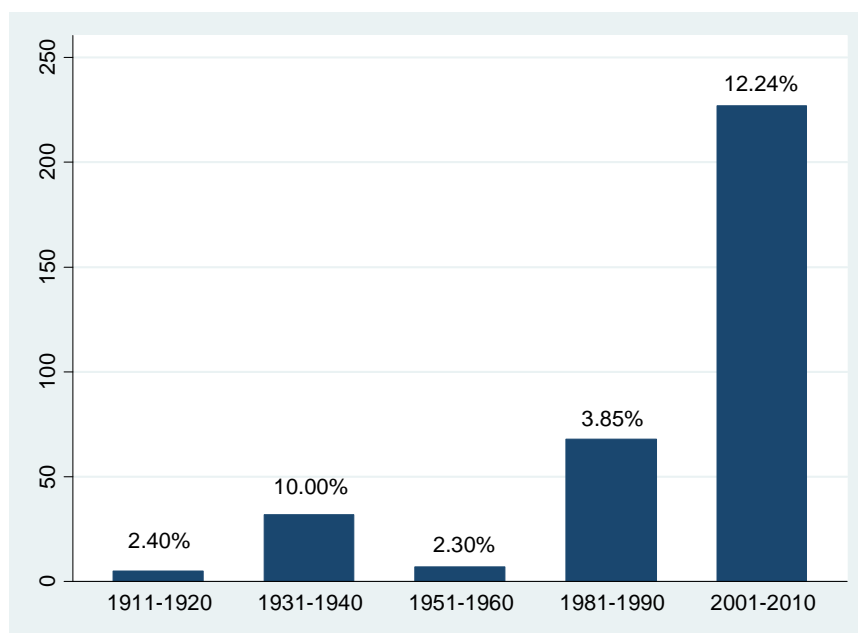


Notes: Number of observations = 865, *AER Paper and Proceedings* included.

4.2 Females Publishing in *AER*

As discussed in the introduction, the first article in the first issue of *AER* was authored by a woman. This admirable beginning, however, was not indicative of things to come (see figure 8). Between 1911 and 1920, female researchers were responsible for only 2.4% of the articles written and, although the proportion of articles authored by women increased to 10% between 1931 and 1940, this momentum was not sustained during the 1950s when only 2.3% of *AER* authors were women. Even in the 1980s, this share only increased to 3.85%. We do, however, observe a significant improvement in female authorship during the 2001 to 2010 period, in which 12.24% of the authors were women.

FIGURE 8: NUMBER AND SHARE OF WOMEN CONTRIBUTING TO *AER*



Notes: Includes only main articles exclusive of those that appeared in *Papers and Proceedings*. We were unable to classify the gender in six cases: one case each in vol. 22 (1932), vol. 24 (1934), vol. 41 (1951), vol. 75 (1985), vol. 89 (1999), and vol. 92 (2002).

TABLE 11: PUBLICATIONS BY WOMEN IN *AER* BETWEEN 1936 AND 1950

Year	# of Females Main Articles	%	# of Females Communications	%	# of Females Book Reviews	%
1936	6	17.14	0	0	4	4.21
1937	4	11.76	0	0	5	4.35
1938	5	13.51	0	0	1	0.89
1939	3	8.11	2	10	8	7.34
1940	2	5.00	0	0	6	6.38
1941	2	5.71	3	6.67	5	4.20
1942	0	0	0	0	9	7.03
1943	2	7.41	0	0	7	5.74
1944	<u>6</u>	<u>24.00</u>	<u>5</u>	<u>22.73</u>	<u>10</u>	<u>8.70</u>
1945	3	11.11	3	8.33	6	5.26
1946	1	3.57	1	3.85	7	8.24
1947	0	0	0	0	4	3.13
1948	0	0	0	0	2	1.71
1949	1	3.45	1	3.13	4	2.76
1950	2	5.56	0	0	8	6.45

Notes: We were unable to identify the gender of a few authors: two in 1938 (book reviews), one two-time contributor in 1941 (book reviews), one in 1944 (communication and book reviews), one in 1948 (book reviews), and one in 1949 (communication).

The noticeably large proportion of female authorship during 1931 and 1940 raises the question of what happened before, during, and after World War II. To answer this question, we first address the general situation faced by economists during the war. W. Allen Wallis (1980: 320), recalling the atmosphere at Stanford soon after the United States's 1942 declaration of war against Japan, Germany, and Italy, throws some light on this issue: "That spring was satirized by a squib in the student paper saying, 'It is rumoured that in the outside world there is a war and a shortage of Coca-Cola.'" At that time, *AER* was under the editorial guidance of Paul T. Homan, who, A. W. Coats (1971: 33) reports, "unlike many editors of scholarly periodicals ... deliberately exercised his editorial initiative by soliciting contributions from the many economists then employed in wartime Washington."⁵⁵ Yet, as

⁵⁵ It may be worthwhile to briefly discuss what actions some of today's economics Nobel Laureates were taking during war. James Tobin was recommended by Edward S. Mason for a job in Washington with the civilian supply division of the nascent Office of Price Administration and Civilian Supply: "So I left Harvard in May 1941, having completed all the requirements for the Ph.D. except the dissertation. I would not return until February 1946. After nine months of helping to ration scarce material, I went to the Navy and served as a line officer on a destroyer until Christmas 1945" (James Tobin 2004: 101–102). Ronald H. Coase remembers that in 1941, "I moved to the Central Statistical Office, one of the Offices of the War Cabinet. I ended up responsible for munitions statistics, those relating to guns, tanks, and ammunition. I did not return to LSE until 1946. My six years in government service played little part in my evolution as an economist, except perhaps to confirm my prejudices" (Ronald H. Coase 2004: 198). Milton Friedman (2004: 72–73) points out that his experience during

table 11 shows, there is no clear observable trend of female engagement during World War II despite some volatility on a yearly basis. However, 1944 clearly marks the period with the largest share of female participation in all sections of *AER* (main articles, communications, and book reviews): 24% of all primary articles were published by female researchers,⁵⁶ who essentially became the “servicewomen of the *AER*.”⁵⁷ Recalling that female contributions during the period between 2001 and 2010 made up only 12% of the total, it is interesting to note that female participation was very low after the war for the years we report.

World War II had a major influence on his scientific work. He spent the years 1941–43 at the U.S. Treasury as an economist in the division dealing with taxes. The second two years of the war (1943–1945) he spent as a mathematical statistician at the Statistical Research Group (SRG) of the Division of War Research of Columbia University: “It had been set up to provide statistical assistance to the military services and to other groups engaged in war research. It was a subsidiary of the wartime-created Office of Scientific Research and Development. Harold Hotelling was its intellectual sponsor and Allen Wallis its executive director. That experience exposed me to physical scientists from a wide range of fields with whom I would otherwise never have had much contact. It also required me to apply statistical techniques to non-economic data. Surprisingly, perhaps, it turned out that social scientists were often more useful than physical scientists in doing operational research that involved interpreting the results of battlefield experience. The reason is simple: social scientists are used to working with bad data and the wartime data were all very bad.” George J. Stigler (2004: 84) stresses that by “1942 the outbreak of war led to a general retrenchment of academic life, and I took a sustained leave of absence from Minnesota, first to the National Bureau of Economic Research. (...) From the bureau I went to the Statistical Research Group at Columbia University, where statistical analysis was being used on military problems. The director was Allen Wallis, and the senior figures included Harold Hotelling, Milton Friedman, Jacob Wolfowitz, and, among other statisticians, L. J. Savage and Abraham Wald. I learned a little statistics there, and I did not seriously delay our nation’s victory.” Other interesting and well-known names also appear among the members of the Statistical Research Group (SRG): Edward Paulson, Julian Bigelow, Abraham Wald, Albert Bowker, Harold Freeman, Rollin Bennett, Jimmie Savage, Kenneth Arnold, Milliard Hastay, Abraham Girshick, Frederick Mosteller, Churchill Eisenhart, and Herbert Solomon. As W. Allen Wallis (1980: 322–323) points out, these economists played an important part: “Fighter planes entered combat with their machine guns loaded according to Jack Wolfowitz’s recommendations about mixing types of ammunition, and maybe the pilots came back or maybe they didn’t. Navy planes launched rockets whose propellants had been accepted by Abe Girshick’s sampling-inspection plans, and maybe the rockets exploded and destroyed our own planes and pilots or maybe they destroyed the target. During the Battle of the Bulge in December 1944, several high-ranking Army officers flew to Washington from the battle, spent a day discussing the best settings on proximity fuzes for air bursts of artillery shells against ground troops, and flew back to the battle to put into effect advice from, among others, Milton Friedman, whose earlier studies of the fuzes had given him extensive and accurate knowledge of the way the fuzes actually performed. We were never wholly responsible for what happened. In fact, we seldom knew whether we were slightly responsible or even knew exactly what happened and to whom. But this kind of responsibility, although rarely spoken of, was always in the atmosphere and exerted a powerful, pervasive, and unrelenting pressure.”

⁵⁶ Simone De Beauvoir, as quoted in David A. Cotter, Joan M. Hermesen, and Reeve Vanneman (2001: 429), made the following remark: “It is through gainful employment that woman has traversed most of the distance that separated her from the male; and nothing else can guarantee her liberty in practice. Once she ceases to be a parasite the system based on her dependence crumbles; between her and the universe there is no longer any need for a masculine mediator.”

⁵⁷ It has been documented and reiterated that the war led to a dramatic increase in the number of women working in the United States, reversing a downward trend attributed to the depression (Marc Miller 1980: 42). More specifically, the number of women working in the U.S. increased from 10.8 million in March 1941 to more than 18 million in August 1944. Although many of these women were entering the labor force for the first time, in 1944–1945, 29% of women already had over 10 years of work experience.

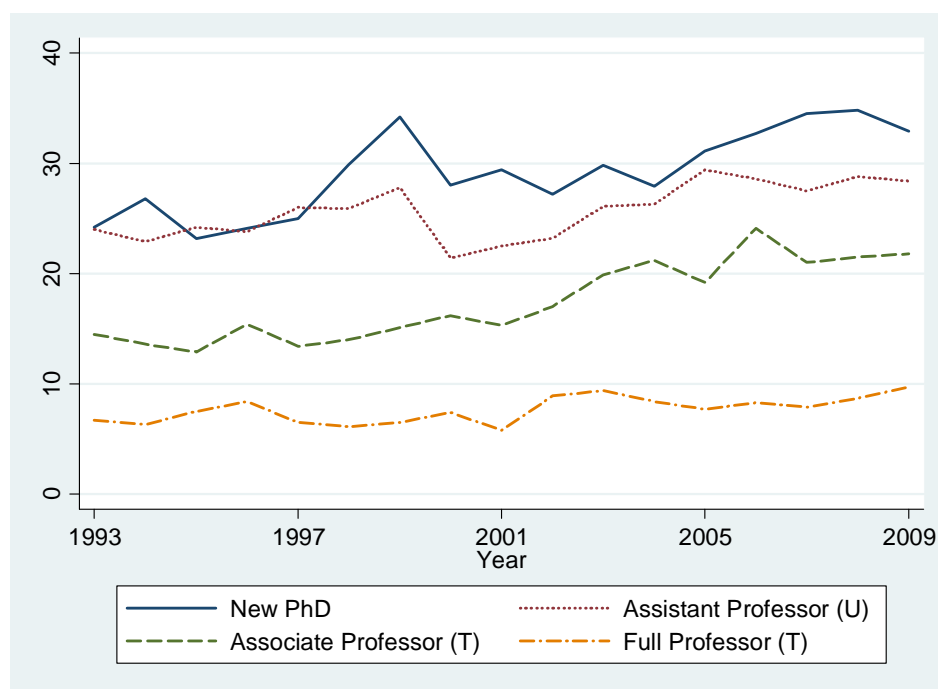
To gain a broader understanding of the position of female researchers in subsequent time periods, it may make sense to take a closer look at the advancement of women in the economics profession. We therefore examine various *Reports of the Committee on the Status of Women in the Economics Profession* (CSWEP) published annually in *AER Papers and Proceedings* since the 1970s, after the AEA decided to promote the progress of women in the profession and passed a resolution to end discrimination against them. The stated purpose of the CSWEP, introduced at the AEA's annual meeting in New Orleans on December 1971, is to monitor the status and promote the advancement of women in the economics profession. As Robin L. Bartlett (1998: 177) points out, "in the late 1960s and early '70s, women were discouraged from studying economics with questions about what they were going to do with a major in economics after they got married. At that time, college-educated women were steered toward teaching certificates or nursing degrees. Some of the country's most prestigious economics departments had higher admission standards for women than they did for men, and admission to a graduate program did not guarantee financial aid. Fellowships or assistantships were disproportionately given to men, since directors of financial aid argued that men were more likely to need the experience to get a good job."

Results from the AEA's *Universal Academic Questionnaire* indicate that the proportion of female assistant professors and associate professors tripled between 1974 and 1989, increasing from 8 to 20% and from 3 to 9%, respectively. The proportion of female full professors, in contrast, grew only from around 2% in the late 1970s to around 3% in the late 1980s (see the *Report of the Committee on the Status of Women in the Economics Profession* published in 1991⁵⁸). These results also indicate that during the 1980s, female assistant professors were more often employed by lower-quality schools, although there was little difference between male and female associate and full professors. In fact, the proportion of

⁵⁸ *AER Papers and Proceedings*, vol. 81, no. 2 (1991): 409–412.

newly hired assistant professors resembled the proportion of new female Ph.D. recipients. These data do however point to a growing pool of women eligible for promotion to associate and full professor in the future. These changes over the last two decades are outlined in Figure 9, which shows an obvious growth trend. Since 1972, when only 8.8% of assistant professors and 3.7% of associate professors were female, the representation of women in the economics profession has increased dramatically, with 21.8% of tenured associate professors being female and the proportion of female full professors rising from 3% in the late 1980s to 9.7% in 2009. Such promising development may help to explain the increase shown in figure 8 of females publishing in *AER* over the last 10 years.

FIGURE 9: SHARE OF FEMALE ACADEMICS IN THE LAST TWO DECADES



In an *AER* paper from 30 years ago, John M. McDowell (1982) discusses the rate of obsolescence or depreciation of knowledge as reflected in the age profile of cited works. To estimate what he terms the “literature decay rate” (over years 1952, 1957, 1962, 1967, and 1972), he compares journals in physics, chemistry, biology, psychology, sociology, history

and English with those in economics and generates data on the proportion of total citations in each that reference publications within the preceding five-year period. He finds the largest decay rate in the relative frequency of citation to older communications in physics and chemistry (an annual average of 18.30), followed by sociology and psychology (10.82), biology (8.68), history (3.85), and English (2.67). For economics journals (including *AER*, the *Journal of Political Economy*, and the *Quarterly Journal of Economics*), he finds an annual average literature decay rate of 13.18 from 1950 through 1974, which suggests that knowledge in economics is relatively less durable than that in other social science fields. Such evidence implies that economics professors who experience a career interruption because of childbearing face high opportunity costs directly related to the rate of skill obsolescence. He also claims that women have responded to the incentives implicit in the differing levels of durable job-related knowledge. More specifically, he relates the proportion of women in specific academic areas to the durability of research knowledge in that area: “A women is twice as likely as a man to specialize in humanities, but only one-third as likely to specialize in physical science. Evidence also suggests that, as fertility rates have declined, women have become more willing to enter nontraditional careers” (761–762). In terms of publication profiles, he shows that married female humanities professors appear to distribute their research publication over a career in patterns similar to those of married men, but that female publication output in nonhumanities fields demonstrates a relative decline during the childbearing years.

Bartlett (1998: 180) also reports that although “the AEA and CSWEP have been successful in eliminating most forms of overt discrimination against women, more needs to be done. A variety of smaller battles have been won. Women have been appointed to prestigious editorial boards and their presence at the national meetings is noticeable. In 1997, three of the six members of the executive committee are women. Seven of the 39 members of

the editorial board of the AER are women, and three of the 18 members of the editorial board of the JEL are women. When the *Journal of Economic Perspectives* was initiated in 1987, only one of its 13 editorial board members was a woman. By 1997, however, women held 3 of the 15 editorial slots on that board.” We wondered, therefore, what the situation is 13 years later. In December 2010, AER’s editorial board included 11 women among its 49 members when all movements throughout the year are taken into account (see appendix table A2), and in January 2011, 10 out of the 20 editorial board members were female. Since the 1980s, however, only 4 AER coeditors have been women,⁵⁹ even though, as previously mentioned, AER now has its first female editor-in-chief (in place since 2011). Female representation in editorial positions for all AEA journals (including the four recently founded) is summarized in table 12, which shows that participation is relatively high in the *Journal of Economic Literature* (JEL). Females, however, are also less likely to be affiliated with the 10 best universities (see table 1).

TABLE 12: AEA JOURNALS AND THEIR SHARE OF FEMALE EDITORS AND COEDITORS

Journal	Females	Females: Top 10 Universities	Males	Males: Top 10 Universities
JEL	10 (43.48%)	4 (36.36%)	13 (56.52%)	7 (63.64%)
JEP	3 (13.04%)	3 (27.27%)	20 (86.96%)	8 (72.73%)
Economic Policy	3 (13.04%)	1 (9.09%)	20 (86.96%)	10 (90.91%)
Applied Economics	3 (13.04%)	2 (18.18%)	20 (86.96%)	9 (81.82%)
Microeconomics	2 (8.7%)	1 (9.09%)	21(91.3%)	10 (90.91%)
Macroeconomics	2 (8.7%)	0 (0%)	21(91.3%)	11 (100%)
Total: All Journals	23 (16.31%)	11 (15.49%)	118 (83.69%)	60 (84.51%)

Notes: The Top 10 universities based on the ranking developed in table 1.

⁵⁹ See http://www.aeaweb.org/aer/AER_Editors_Coeditors.pdf.

5. *AER* Editors, Coeditors, Board Members and Referees

Given that the spotlight so far has been on the glory of the authors who managed to publish in *AER*, it might now be interesting to also go backstage. We therefore now clear the stage and raise the curtain on the editors, coeditors, and referees to reveal (among other things) their individual profiles. In examining the growing literature on “publishing economics,”⁶⁰ we note that the evaluation of editor, coeditor, editorial board member, and referee *characteristics* is relatively less developed. This lack of accessible information was emphasized several years ago by Daniel S. Hamermesh (1994: 153), who, commenting on referees, pointed out that we know “very little about who they are.” Because the names of *AER* editors, coeditors, editorial board members, and referees are now transparently available, however, we were able to collect a wide variety of individual characteristics from the CVs available on the Web. Most particularly, we borrowed a technique from the motion picture industry, considered the major *information industry* of the twentieth century (see Arthur De Vany’s (2004) *Hollywood Economics*); namely, using awards as a symbol of recognition.⁶¹ Accordingly, assuming academia to be a *true source of information and knowledge*, we round out this section by devoting some time to the empirical data on awards.

5.1 Editors

AER editors have not only shaped academic knowledge through their editorial work but have also influenced both their academic surroundings and the policy landscape beyond. For

⁶⁰ For example, for a great selection of 15 papers, see *Publishing Economics*, edited by Joshua Gans (2000).

⁶¹ Samuelson (2004: 60) expounds on this theme in his section Chasing the Bitch Goddess of Success: “Let me close with a few remarks on the motivation and rewards of scientists. Scientists are as avaricious and competitive as Smithian businessmen. The coin they seek is not apples, nuts, and yachts; nor is it the coin itself, or power as that term is ordinarily used. Scholars seek fame. The fame they see, as I noted in my 1961 American Economic Association presidential address, is fame with their peers—the other scientists whom they respect and whose respect they strive for. The sociologists Robert K. Merton has documented what I call this dirty little secret in his book *The Sociology of Science*. I am no exception. Abraham Lincoln’s law partner and biographer William Herndon observed that there was always a little clock of ambition ticking in the bosom of honest and whimsical Abe. No celebrity as a *Newsweek* columnist, no millions of clever-begotten speculative gains, no power as the Svengali or Rasputin to the prince and president could count as a pennyweight in my balance of worth against the prospect of recognition for having contributed to the empire of science.”

example, Davis R. Dewey shaped MIT's graduate course structure and played a crucial role in expanding its economics department,⁶² while Bernard F. Haley had a significant impact on Stanford's economics department.⁶³ Likewise, Ben S. Bernanke, editor of *AER* between 2001 and 2004, is currently chair of the United States Federal Reserve after previously serving as its governor and as chair of President George W. Bush's Council of Economic Advisers.⁶⁴

The first *AER* editor, Davis R. Dewey, born in Burlington, Vermont, on April 7, 1858, had the longest editorial reign in *AER*'s history, occupying the post for 30 years from 1911 to 1940. It may therefore be interesting to have Dewey return to the stage, by revisiting his speech at the testimonial dinner that marked the ceremonial transfer of his editorial blue pencil to Paul T. Homan ("Remarks" 1941):⁶⁵

May I now give a few words of advice to our new managing editor?⁶⁶

- 1) Be sure to have one article containing involved mathematical equations with unusual fonts of type. Inasmuch as the printer has to spend a good deal of time in ransacking the type foundries of the country, this affords you a good excuse for a delay in publication.
- 2) Be sure that a majority of the leading articles contain at least six references to Keynes. Adam Smith, John Stuart Mill, Marshall, Francis A. Walker and their contemporaries are now

⁶² See <http://econ-www.mit.edu/about/>.

⁶³ Tibor Scitovsky, who was recruited to Stanford by Haley after World War II, stated that: "Stanford's economics department wasn't very good before the war, and he was tremendously successful at building it" (see "Economics Department architect Bernard Haley dies at 94," Stanford News Release, June 8, 1993: available at <http://news.stanford.edu/pr/93/930608Arc3228.html>).

⁶⁴ See http://en.wikipedia.org/wiki/Ben_Bernanke.

⁶⁵ He began his speech with, "Your tribute has been so overwhelming that I find it difficult to speak. However, in order to restore my tranquility I shall tell a story. A Vermont farmer was leading a calf along a road until they came to a bridge. There the calf balked. An automobile came along, but the calf blocked the way. Finally the farmer said to the chauffeur, 'If you will toot the horn maybe the calf will move.' The chauffeur blew the horn and the calf jumped clear off the bridge into the water and was drowned. The farmer said, 'What did you do that for?' The chauffeur replied, 'You told me to blow.' 'Yes,' said the farmer, 'but wasn't that a hell of a big toot for a little bit of a calf?'"

⁶⁶ Coats (1969: 62) points out that "Paul Homan professed to be 'bursting with ideas' when he took up the reins of office of Managing Editor at the age of forty-seven and there is no doubt that he transformed the *A.E.R.* during his eleven-year tenure. From the outset he introduced a personal note which had hitherto been lacking, addressing himself directly to his readers in occasional editorial notes and in the annual reports, inviting advice and criticism, and disclosing his plans for improving the content and the format of the journal. Circumstances were difficult owing to the outbreak of the war, which inevitably disrupted academic life, and to the fact that he moved several times between Ithaca, Washington, and London before settling in Los Angeles in 1950s." He also points out that the change to Paul Homan was welcome: "Dissatisfaction with the character and contents of the *A.E.R.* had been growing for some time, and as Dewey was eighty-two when he retired, it is hardly surprising that his efficiency had declined and that he was out of touch with the ideas and needs of the younger generation of economists."

passé. And it is your duty to see that the articles you select do not burden the readers with reasoning which has been outmoded.

- 3) Publish at least one review in each issue which will arouse the animosity of the author. There is nothing more stimulating than controversy. Is not controversy the essence of that much debated theme, democracy? (...)
- 4) Be sure to have occasionally an article contain fifty-cent and one-dollar words. Though difficult to understand, such an article commands respect; and economists in these days need respect.⁶⁷

What, then, are the requirements of becoming an *AER* editor? Coats (1969: 65) recalls the process involved in selecting Paul T. Homan as the second editor of *AER*. AEA's president at that time, Frank H. Knight,⁶⁸ describes the process as "worse than making a

⁶⁷ *American Economic Review*, vol. 30, no. 5 (February 1941): vii–xi.

⁶⁸ For those few readers who do not remember Frank H. Knight, below are some telling statements about him made by several Nobel Laureates. Samuelson (2004: 50) refers to "Frank Knight and Jacob Viner, my great neoclassical teachers in Chicago," while Friedman (2004: 70) recalls that "[o]ther faculty members at Chicago included Frank Knight, Henry Simons, Lloyd Mints, Paul Douglas, and Henry Schultz. Economists will recognize their names; the rest of you will not." James M. Buchanan (2004: 140) also reminisces about Knight: "During the first quarter I took courses with Frank Knight, T.W. Schultz, and Simeon Leland. I was among the very first group of graduate students to return to the academy after discharge from military service during World War II. We swelled the ranks of the graduate classes at Chicago and elsewhere. Within a few short weeks, perhaps by mid-February 1946, I had undergone a conversion in my understanding of how an economy operates. (...) For the first time I was indeed an economist. I attribute this conversion directly to Frank Knight's teaching, which perhaps raises more new questions than it answers." Later, Buchanan writes, "In the classroom he came across as a man engaged always in a search for ideas. He puzzled over principles, from the commonsensical to the esoteric, and he stood continuously dismayed at the arrogance of those who spouted forth the learned wisdom. Knight gave those of us who bothered to listen the abiding notion that all is up for intellectual grabs, that much of what paraded as truth was highly questionable, and that the hallmark of a scholar was courage in cutting through the intellectual haze. The willingness to deny all gods, to hold nothing sacrosanct—these were the qualities of mind and a character that best describe Frank Knight. And gods, as I use the term here, include the authorities in one's own discipline as well as those who claim domain over other dimensions of truth. Those of us who were so often confused in so many things were bolstered by this Knightian stance before all gods. Only gradually, and much later, did we come to realize that in these qualities it was Frank Knight, not his peers, who attained the rank of genius. As he was the first to acknowledge, Frank Knight was not a clever or brilliant thinker. He was an inveterate puzzler; but his thought process probed depths that the scholars about him could not realize even to exist. To Knight, things were never so simple as they seemed, and he remained at base tolerant in the extreme because he sensed the elements of truth in all principles. (...) Knight was the advisor who told me not to waste my time taking formal courses in philosophy, who corrected my dissertation grammar in great detail, and who became the role model that has never been replaced or even slightly dislodged over a long academic career. In trying to assess my own development, I find it impossible to imagine what I might have been and become without exposure to Frank Knight" (144). Ronald H. Coase (1994: 195) was also influenced by Knight: "At Dundee I began to read the literature of economics—Adam Smith, Babbage, Jevons, Wicksteed, Knight," as was Gary S. Becker (2004: 256): "I stayed at Chicago for six years, the first three as a graduate student. During the second year I was looking for a thesis topic and had already done some research on an economic approach to political democracy. My paper on this topic was almost published in the *Journal of Political Economy*, but one of my teachers, Frank Knight, was the referee, and he did not like it. I have kept his comments to this day. Knight was a great economist, but he looked at democracy with what I would characterize as a normative point of view. He defined democracy as government by discussion." Finally, George J. Stigler (2004: 81) too recalls Knight positively: "There I met and got to know three economists I still consider to be outstanding: Frank Knight and Henry Simons, and a year later, on his return for the U.S. Treasury, Jacob Viner. Knight was both a great and an absurd teacher. The absurdity was documented by his utterly disorganized teaching, with constant change of subject and yet insistent repetition of arguments. In the course on the history of economics he was interested mostly in the seamy side of religious history, but got great relish out of

major appointment to the faculty and that's bad enough". The requirements for a successful editor were laid down by one of the selectors: "[He] should be someone who has already made something of a reputation; he should neither be too young nor too old (say 40 to 55); he should have a good background in theory (which after all is the common meeting ground of all the special fields of economics). He should command respect for character and judgment, as well as proficiency in some of the more decent parts of economics; he should have some enthusiasm for the job of editor; and he should have good prospects of lasting (from the age [sic. angle?] of age, health, and stability of character, for a decade or so). A journal stands to gain a good deal from continuity in its direction" (Coats 1969: 65).⁶⁹

To assess the applicability of these requirements, we now test them *ex post*. However, because of problems with degrees of freedom ($N = 9$) and data availability, we do so using simple case studies on the three latest male *AER* editors, Robert A. Moffitt, Ben S. Bernanke, and Orley C. Ashenfelter. We choose to exclude Pinelopi Koujianou Goldberg (who became editor in 2011)⁷⁰ to be consistent with the original's reference to "a man" as a potential candidate. The data are derived mainly from these scholars' CVs to produce the profiles summarized in table 13.⁷¹ In terms of the first point that the editor "should be someone who

emphasizing the perversities and blunders of Ricardo and other historic figures in economics. His greatness is attested best by the fact that almost all the students were much influenced by him. He communicated beyond any possible confusion the message that intellectual inquiry was a sacred calling, excruciatingly difficult for even the best of scholars to pursue with complete fidelity to truth and evidence." He adds, "One thing that Knight and Simons both succeeded in teaching me, and in fact overtaught, was that great reputation and high office deserve little respect in scientific work. We were told to listen to the argument and look at the evidence, but ignore the position, degrees, and age of the speaker" (82). He also explains that "I wrote my dissertation in the history of economic thought under Knight. He was the soul of kindness and generosity in dealing with me, then and forever after but in retrospect there was a fly in the ointment. He was so strong-minded and so critical a student of the literature that it was a good many years before I could read the economic classes through my eyes instead of his. I have never brought myself to read through my doctoral dissertation, *Production and Distribution Theories: The Formative Period*, because I knew I would be embarrassed by both its Knightian excesses and its immaturity (83)" Lastly, he admits that "I am no longer a faithful follower, although I am still an admirer, of Frank Knight and Henry Simons: each person has a mind-style of his own, and eventually it asserts itself. This does not mean that we are immune to our environment, but it does argue for me that environmental influences will be subtle" (93).

⁶⁹ Bernard Haley was chosen for fulfilling these requirements.

⁷⁰ Dr. Goldberg's background data was evaluated when she was considered for her prior position as *AER* coeditor (2007–2010, see figures 10, 11, and 12).

⁷¹ As regards awards, *AER* publications, publications in *QJE*, *JPE*, and *Econometrica*, and total journal publications, we focus on the period before these individuals began work as *AER* editor.

has already made something of a reputation,” such is clearly the case for all three editors. All three, for example, became Fellows of the Econometric Society several years prior to accepting their positions as editor, one (Ashenfelter) was already a Fellow of the American Academy of Arts and Science (AAAS), and another (Bernanke) became an AAAS fellow the same year he began as *AER* editor. All three published widely *before* their appointment (between 47 and 102 refereed journal articles) , including several publications in *AER* (between 4 and 9 including the *Papers and Proceedings*) and other top economics journals (between 6 and 9 in *QJE*, *JPE*, and *Econometrica*). According to their current positioning on IDEAS/RePEc,⁷² all three are ranked among the top 200 researchers worldwide.

The next requirement for an *AER* editor, that “he should neither be too young nor too old (say 40 to 55),” also seems to hold up. Ashenfelter, born in 1942, began his position as editor in 1985 and was therefore in his early forties; Bernanke, born in 1953, started in 2001 and was therefore in his late forties; and Moffitt, born in 1948, took the position in 2004, and was therefore in his mid-fifties. Likewise, all three appear to meet the third requirement that “[the editor] should have a good background in theory (which after all is the common meeting ground of all the special fields of economics). He should command respect for character and judgment, as well as proficiency in some of the more decent parts of economics.” Prior to their respective appointments, all three editors had carried out important work on a large variety of issues that reflected a strong background in theory. The publication proficiency of each editor is apparent in the successes discussed earlier, and the question of whether they met the standards laid down by their predecessor is well illustrated by their peers’ evaluation of their work. One simple way to measure this factor is to examine the *content* of award speeches or statements, a technique for which Ashenfelter provides a good case study. When

⁷² Data for the time at which they took over the editorial role were unavailable.

he received the 2003 IZA Prize in Labor Economics,⁷³ IZA Director Klaus F. Zimmermann introduced him as “one of the most influential economists of our time, [one who had] played a crucial role in the evolution of modern empirical labor economics, especially excelling in the methodological advancement of evaluation research.” The award statement also points out that Ashenfelter’s scholarly contributions have made him “one of the most influential architects of modern labor economics” and that his “intellectual work stands out due to his ingenuity in devising clever ways to derive and test hypotheses of economic models, his exceptional creativity in using and collecting data, and his originality in pioneering the natural experiment methodology.” It also identifies him as “the founding father of what has by today developed into the separate fields of quantitative social program evaluation.”

The criteria for a successful editor also demand that “he should have some enthusiasm for the job of editor.” As table 13 clearly shows, all three editors had remarkable prior editorial accomplishments in excellent and often specialized journals, which may indicate a preexisting high level of enthusiasm for the editorial job. For example, Moffitt had been associate editor of *AER*, deputy editor of *Demography*, coeditor/associate editor of the *Review of Economics and Statistics*, associate editor of *Labour Economics*, associate editor of the *Journal of Public Economics*, chief editor of the *Journal of Human Resources*, associate editor of the *Journal of Population Economics*, and associate editor of the *Journal of Business and Economic Statistics*. It is therefore hard to imagine that he did not like editorial work.

There is but one remaining criterion from the statement of requirements: “...and he should have good prospects of lasting (from the age [sic. angle?] of age, health, and stability of character, for a decade or so).” Evaluation of this criterion we leave to family members, friends, colleagues, and loved ones. We must point out, however, that although these individuals are no longer editors of *AER*, they seem, to the outside observer, quite alive!

⁷³ See *IZA Compact*, November/December 2003: http://ftp.iza.org/compacts/iza_compact_en_16.pdf.

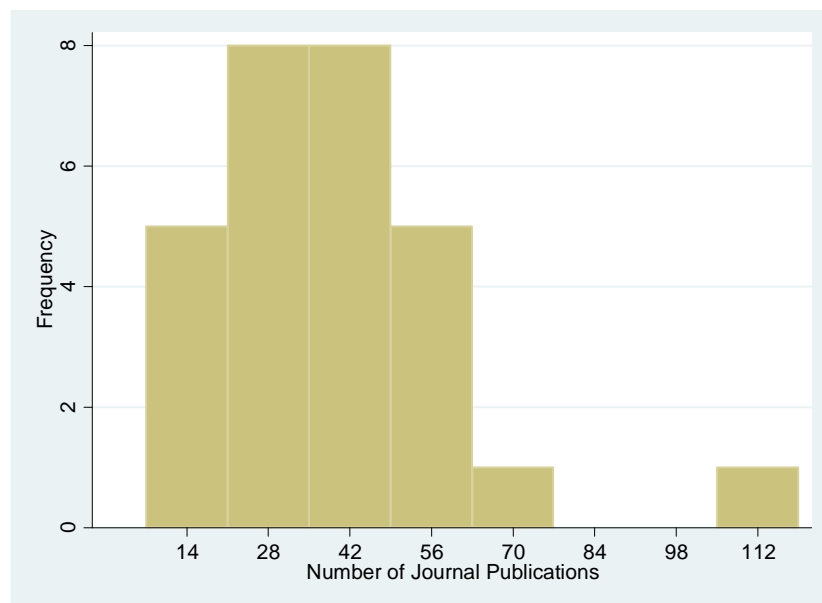
TABLE 13: PROFILE OF *AER* EDITORS BETWEEN 1985 AND 2010 BASED ON THEIR CHARACTERISTICS
A YEAR BEFORE STARTING AS EDITOR

NAME	ROBERT A. MOFFITT	BEN S. BERNANKE	ORLEY ASHENFELTER
TERM	2004–2010	2001–2004	1985–2001
AFFILIATION	The Johns Hopkins University	Princeton University	Princeton University
BORN	1948	1953	1942
Ph.D. (YEAR)	Brown University (1975)	MIT (1979)	Princeton University (1970)
AWARDS: FELLOW, ECONOMETRIC SOCIETY	YES, 1997	YES, 1997	YES, 1977
FELLOW, AMERICAN ACADEMY OF ARTS AND SCIENCE	NO	YES, 2001	YES, 1993
<i>AER</i> PUBLICATIONS INCLUDING PROCEEDINGS	5	9	4
<i>AER</i> PUBLICATIONS IN THE PAPERS AND PROCEEDINGS	3	5	1
PUBLICATIONS IN <i>QJE</i> , <i>JPE</i> AND <i>ECONOMETRICA</i>	6	8	9
TOTAL JOURNAL PUBLICATIONS BEFORE EDITORSHIP	102	62	47
PAST EDITORIAL EXPERIENCE <i>AER</i>	1995–2001, Associate Editor	None before being editor	2001–2002, Coeditor, <i>AER</i>
OTHER EDITORIAL EXPERIENCE	1993–1995, Deputy editor, <i>Demography</i> 1991–1998, Coeditor, <i>Review of Economics and Statistics</i> 1991–1997, Associate editor, <i>Labour Economics</i> 1988–1997, Associate editor, <i>Journal of Public Economics</i> 1988–1991, Chief editor, <i>Journal of Human Resources</i> 1987–2006, Associate editor, <i>Journal of Population Economics</i> 1987–1992, Associate editor, <i>Journal of Business and Economic Statistics</i> 1986–1991, Associate editor, <i>Review of Economics and Statistics</i>	1994–2001, Coeditor, <i>NBER Macroeconomics Annual</i> 1993–96, Coeditor, <i>Economics Letters</i> 1990– present, Associate editor, <i>Journal of Financial Intermediation</i> 1993, Associate editor, <i>Journal of Money, Credit, and Banking</i> 1985–1992, Associate editor, <i>Quarterly Journal of Economics</i> 1993, Associate editor, <i>Review of Economics and Statistics</i>	Cofounder of the <i>American Law and Economics Review</i>
IDEAS RANKING POSITION (NOVEMBER 2010)	166	23	190

5.2 Coeditors

We now take the analysis one step further by looking more closely at *AER* coeditors, who, because of the quantity of papers submitted, play a crucial role in the journal's success. Such a large share of submissions may also increase the need to guarantee that coeditors have a higher level of autonomy in their decision process.⁷⁴ Although the three editors discussed above have also been coeditors, we exclude them from this analysis, which gives us a total sample of 26 individuals. As appendix table A3 shows, 8 of these coeditors earned their doctorates at MIT,⁷⁵ 5 have a Ph.D. from Stanford University, and 73% have a Ph.D. from an institution that ranks among the 10 best universities listed in table 1, with which 42% of the coeditors are still affiliated.

FIGURE 10: NUMBER OF JOURNAL PUBLICATIONS BEFORE BECOMING *AER* COEDITOR



⁷⁴ See, for example, the *Public Choice* literature on local autonomy or federalism.

⁷⁵ The MIT dominance is consistent with the results reported in table 1 and table 5.

In terms of *quantity* of journal publications, however, figure 10 shows a certain diversity: the number of publications before being appointed coeditor ranges from only 14 to 111⁷⁶. On average, an *AER* coeditor has 39 such publications (std. dev. = 21.24). To check not only for quantity but also for quality, in the appendix, we report the number of publications in *AER* (figure A1) and those in the other top journals *QJE*, *JPE*, and *Econometrica* (figure A2). Doing so reveals a certain level of heterogeneity: on average, each researcher published 4.5 *AER* publications before becoming coeditor (std. dev. = 3.04) and close to 6 publications in *QJE*, *JPE*, and *Econometrica* (std. dev. = 3.12). We also check their overall IDEAS rankings for November 2010 (post analysis) and find that, apart from the two coeditors who are not registered, all coeditors are in the top 5%. Nonetheless, as figure A3 shows, their rankings differ substantially, from position 12 to position 1223 (std. dev. = 375; mean = 385).

Figure 11 reports the link between the coeditors' academic age (first year as coeditor - year Ph.D. obtained), and, in line with the previously mentioned figures, we find substantial heterogeneity. Academic age ranges from 7 to 32, with a mean value of 17 (std. dev. = 6.88), indicating that on average, *AER* coeditors do have substantial academic experience before their appointment to the position.

Next, in figure 12, we report the level of editorial experience gained prior to becoming *AER* coeditor, with attention to a variety of past involvement in other journals (e.g., as editor, coeditor, guest editor, editorial board member). Again, we observe substantial differences among coeditors: from no experience at all to a relatively active level of engagement (mean = 4.16).

⁷⁶ The number of journal articles published (including *AER* publications); we exclude book reviews, chapters in published books, or complete books.

FIGURE 11: ACADEMIC AGE OF *AER* COEDITORS

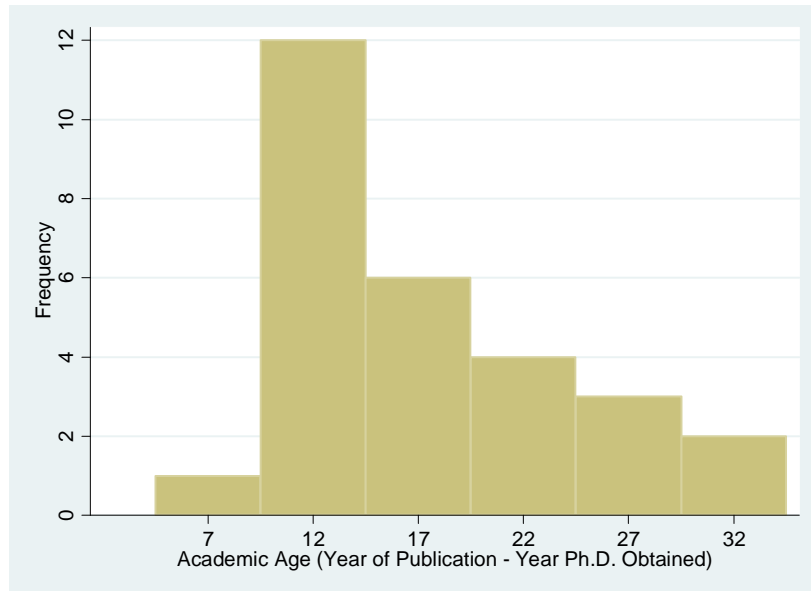
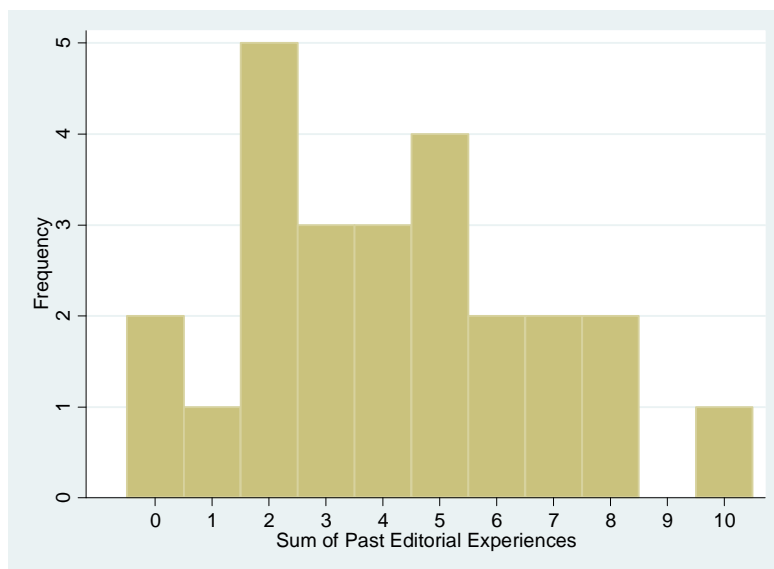


FIGURE 12: COEDITORS' EDITORIAL EXPERIENCE



Notes: Covers all the coeditors' former editorial experience before starting the position as *AER* coeditor (editor, associate editor at a journal other than *AER*, editorial board member, guest editor, and so forth), except for the three editors (and former coeditors) analyzed in the previous section.

5.3 *Connections between Editors and Coeditors and Citation Success*

The existence of an editorial favoritism, a so-called “connection award,” is the subject of frequent discussion. The conferral of such favoritism, however, is obviously far less transparent. Daniel L. Sherrell, Joseph Hair, Jr., and Mitch Griffin (as cited in David N. Laband and Michael J. Piette 1994: 194–195), in their survey of 15 editorial practices among 328 marketing faculties, identify two that are regarded as the least ethical: “(1) favoritism to friends and personal associates by an editor or reviewer and (2) selection of reviewers that have a strong bias (pro or con the manuscript’s content area and methodology) in order to ensure acceptance or rejection.” Laband and Piette (1994: 195) thus suspect a belief by academic scholars that journal editors should be “disinterested gatekeepers” rather than “self-interested deal cutters.” They suggest, however, that this “shared perception of scientific advancement as a steady progression, however slow, from error to truth is an illusion that is seemingly shattered by the practice of editorial favoritism” (195). Moreover, such “editorial favoritism implies a wealth redistribution in favor of certain members of the scientific community at the expense of other members,” one to which these members “are liable to object vociferously” (195). In fact, Geoffrey M. Hodgson and Harry Rothman (1999: F172), in a study in the *Economic Journal*, claim that their results show clear evidence “of a domination of journal articles and editorships in economics by just a few U.S. academic institutions. Clearly, this evidence raises disturbing questions about the existence of an ‘oligopoly’ of U.S. institutions dominating leading journals in economics and economics research throughout the world. This dominance manifests itself both in terms of editorial control and in terms of highly cited journal articles.” In drawing conclusions from their results, they ask the following question: “Is there evidence here of ‘favouritism’ by editors on

behalf of authors with similar institutional origins or connections? Such a causal connection is not proven. The clearest evidence of editorial favouritism towards specific institutions in the selection of articles for publication would depend upon a comparison of data on the institutional origins of *submissions*, with the institutional origins of *acceptances*. If the institutional (or country) pattern of submissions was very different from that for the institutional (or country) locations or origins of editors then there would be a case for further investigation. It might suggest the possibility of institutional bias. Regrettably, however, no general evidence is available on the institutional affiliation of the authors of all articles *submitted* to journals” (F180). They also admit, however, that apart “from favouritism, a host of other explanations are possible. Some of these may involve path-dependent processes. Institutions with an initial concentration of editors or authors may benefit from processes of positive feedback involving, for example, an increasing capacity to recruit leading researchers, and increasing research output” (F180). However, in line with other researchers such as David Colander (1994)⁷⁷, they refer to the importance of diversity : “There are grounds to presume that the dominance of the profession by a few leading institutions is likely to reduce the diversity in approaches and beliefs” (F180–F181). Laband and Piette (1994: 195–196) further point out that there “are two competing arguments about the nature of the relationship between quality of papers published in scientific journals and personal ties between the author(s) and the editor. On one hand, editors may publish substandard papers

⁷⁷ “The economics profession is not in a crisis. It is simply in a slow decline, as is suggested by the declining number of US citizens receiving Ph.D.s in economics over the last twenty years. Eventually, the problems in the profession will cause the current institutional structure to break down, or to change, to better accommodate disagreement in judgment. But any change is unlikely to occur anytime soon. Nonetheless, the current institutional structure of the profession has short-run costs. To be sincere in one's disagreements, as I believe economists are, and simultaneously to hide the true nature of the disagreement requires a certain detachment from the analysis. Hiding the true nature of the disagreement makes it impossible to arrive at intuitively satisfying resolutions to debates. Moreover, it makes the resulting research less valuable than it could be. Another effect of the institutional structure in the profession is that it strongly discourages disagreement based on judgment and sensibility, where much of the disagreement about economic theory and policy resides, and that it encourages economists to surround themselves with like-minded economists, rather than encouraging interaction and debate with economists who have differing sensibilities and judgments. This leads to geographical pockets of agreement” (David Colander 1994: 54).

written by their personal friends or professional allies. That is to say, the publication standard applied by the editor to papers submitted by these individuals is lower than the one that must be met by individuals with no connection to the editor, and perhaps substantially lower than the standard that must be met by individuals whom the editor dislikes. We are painfully aware that we have no ready answer to the question ‘Why would editors do this?’ except to emphasize that, to our knowledge, no widely accepted theory of editorial behavior has ever been articulated. This difficulty notwithstanding, editorial favoritism of this form implies that papers authored by individuals with personal ties to the editor will be of lower quality than those written by individuals unconnected to the editor, *ceteris paribus*. In stark contrast to this view stands the argument made to us by numerous journal editors (independently) over the years: the personal feelings of authors to the contrary notwithstanding, there is a consistent shortage of truly good papers authored by scholars in economics. Journal editors compete to identify and publish the (few) papers with relatively great substantive impact.”

To explore this question empirically, Laband and Piette (1994) explore the extent to which an author’s personal ties to the editor of a journal influence subsequent citations to published articles using an impressive data set of 1,051 full articles published in 28 top economics journals in 1984. They define “an author/editor connection to exist whenever any of the authors of an article received his or her Ph.D. from the same university that the editor, coeditor, or any associate editor of the journal that published the paper was affiliated with in 1984 or received his or her Ph.D. degree from, or if any of the authors of a paper was affiliated in 1984 with the same university that the editor, coeditor, or any associate editor was affiliated with in 1984 or received his or her Ph.D. degree from” (197). After controlling for author gender, mean age, stock of citations 1979–1983, article (length, whether a lead article), and journal-specific characteristics (journal quality), they find that connections have a highly significant *positive not negative* impact. This finding, the authors argue, “provides

empirical support for the contention that the editorial process is competitive and that editors use their connections to actively search out high-impact papers for publication in their journals” (199). They also point out that it “seems possible, if not probable, that part of the implicit compensation offered to journal editors is the opportunity to publish low-quality papers, relatively speaking, written by professional friends (including himself) and allies. Indeed, to the extent that an editor can arrange quid pro quos in the form of invitations to give paid lectures, attend prestigious conferences, join esteemed societies, and the like, this prerogative may, on the margin, be one of the more powerful inducements motivating the supply of editors. If this prerogative were denied journal editors, either the quality of editors would decline or we would have to pay more for subscriptions” (202).

Motivated by their study, we investigate the relationship between connections and citations by focusing on *AER* articles published between 1984 and 1988. For the dependent variable, we use the number of citations accumulated up until September 2008 as reported by *Journal Citation Reports*. We identify and report separately three author/editor connection, which we define as authors who are departmental colleagues of editors or coeditors, who obtained their Ph.D. from the same university as the editors and coeditors, and who share an editor’s or coeditor’s former university affiliation.⁷⁸ In this way, we are able to explore three author/editor connection variables. The results of a similar analysis focusing on the first two variables by Laband and Piette (1994: 201) remain unreported, however, these authors do suggest that according to their results, “editors’ colleagues, not former graduate students of the department” are “the source of the most heavily cited papers.”

In line with the methodology employed by Laband and Piette, we control for length of paper, whether or not the paper is a lead article, and author gender;⁷⁹ however, rather than

⁷⁸ The *AER* editors during our period of investigation were Robert W. Clower (1981–1985) and Orley Ashenfelter (1985–2001); the coeditors were John G. Riley (1983–1987), John B. Taylor (1985–1988), Robert H. Haveman (1985–1991), Hal R. Varian (1987–1989), and Bennett T. McCallum (1988–1991).

⁷⁹ Share of females.

focusing on the authors' mean age, to avoid missing values, we use their mean academic age (year of publication – year Ph.D. obtained). To estimate a proxy for author quality, we focus not on the author's stock of citations in previous years but, in the spirit of our *AER* investigation, on the number of former *AER* publications (excluding *Paper and Proceedings* articles) although we realize that this proxy may be criticized. The focus on *AER* publications alone means that, unlike Laband and Piette, we need not control for journal quality. We do, however, control for several additional factors that Laband and Piette do not take into account: the number of coauthors (one, two, three or more, with single-authored contributions as the reference group), the level of technique used in the articles (number of equations, tables, figures, and references), and the article's subject-areas based on the JEL code classification system. In addition, because a paper published in 1984 has more time to attract citations than a paper published in 1988 (defined as the reference group year), we include year dummies. We also include a dummy for main articles as we also consider short papers. In line with Laband and Piette, however, we report OLS results for the number of citations as the dependent variable.

We first explore the three single-connection factors independently in specifications 1, 2 and 3 and then report them jointly in specifications 4 to 7 (including always only one affiliation proxy), the second of which (specifications 6 and 7) includes the JEL subject areas. Our results, given in tables 14a and 14b, show a *positive* relationship for the editor/author connection based on graduate school (doctoral institution) that is weakly statistically significant. On the other hand, the connection coefficients for university affiliation (current and past) are not statistically significant, albeit negative for current affiliations and positive for past affiliations. These results do not support the notion that connections help authors publish papers that are lower in quality based on the assumption that citations are a good proxy for quality. Rather, in line with previous results like those presented in tables 1 and 5,

they may indicate that graduate programs in these top universities provide their graduates with a solid foundation for survival in the academic landscape. Thus, although our findings confirm Laband and Piette's primary finding of favoritism, they identify the connection between editors and the former graduate students of the departments in which they obtained their doctorates as the source of the most heavily cited papers. Interestingly, in the late 1990s, *AER* introduced a policy to avoid such conflicts of interest: in May 1998, the Report of the Editor, published in the *Papers and Proceedings*, stated that as "a general rule, editors are never assigned papers written by authors at the same institution" (511).⁸⁰

The control variables also reveal some interesting results. When, rather than focusing entirely on lead articles, we use a dummy for main articles,⁸¹ we find a positive correlation between primary article and citation success once length of the paper is controlled for. Moreover, we find a negative correlation between lead articles and subsequent citation success. In all estimations, the coefficient is statistically significant at the 10% level. Thus, assuming that *AER* editors care about the order of the main articles, we, unlike Laband and Piette (1994: 198–199), find no support for the claim that "editors correctly select high-impact papers for publication as lead articles."

⁸⁰ The May 2005 Report of the Editor provides an extension: "Co-Editors are not assigned papers written by authors at their own institution, or papers written by individuals with whom the Co-Editor has a close professional relationship, most often a present or past co-author relationship. Such papers are handled by other Co-Editors or the Editor. In addition, all submissions by Co-Editors themselves are handled by the Editor" (485). A year later, the statements were extended again: "Coeditors are generally not assigned manuscripts authored by an individual at his or her institution, by an individual with whom the Coeditor has been a recent coauthor, by an individual who has a close professional or personal relationship with the Coeditor, or by an individual who has served as a graduate student advisor or advisee of the Coeditor. Papers falling into these categories are handled by the Editor or by a different Coeditor with appropriate procedures for confidentiality of refereeing. Papers submitted by a Coeditor are handled by the Editor and papers submitted by the Editor are handled by a Coeditor, again employing appropriate confidentiality procedures" (497).

⁸¹ Other short articles such as notes, replies, or comments make up the reference group.

TABLE 14A: CONNECTIONS AND CITATIONS SUCCESS

Independent Variables	Dependent Variable: Total Citations		
	[1]	[2]	[3]
University Connection (Current Affiliations)	-1.294 <i>-0.07</i>		
University Connection (Past Affiliations)		3.038 <i>0.22</i>	
PhD University Connection			21.117* <i>1.75</i>
Length	4.211*** <i>2.67</i>	4.165*** <i>2.61</i>	4.066** <i>2.51</i>
Main Article (Dummy)	19.112** <i>2.54</i>	18.804** <i>2.36</i>	19.218*** <i>2.58</i>
First Article (Dummy)	-29.45* <i>-1.83</i>	-29.702* <i>-1.84</i>	-29.039* <i>1.82</i>
Female Share	-0.361 <i>-0.04</i>	-0.189 <i>-0.02</i>	1.561 <i>0.17</i>
Academic Age	-0.65 <i>-1.25</i>	-0.618 <i>-1.17</i>	-0.479 <i>-1.01</i>
Authors' Quality	5.987* <i>1.71</i>	5.80 <i>1.61</i>	5.448 <i>1.62</i>
Equations Main Text	-0.41 <i>-1.33</i>	-0.40 <i>-1.29</i>	-0.422 <i>-1.35</i>
Tables Main Text	-0.188 <i>-0.08</i>	-0.169 <i>-0.07</i>	-0.187 <i>-0.08</i>
Figures Main Text	7.024* <i>1.81</i>	7.008* <i>1.79</i>	6.57* <i>1.73</i>
References per Article	0.736 <i>1.26</i>	0.747 <i>1.28</i>	0.77 <i>1.31</i>
Year 1984	13.605 <i>1.13</i>	14.242 <i>1.17</i>	13.174 <i>1.12</i>
Year 1985	18.065 <i>1.59</i>	18.314 <i>1.49</i>	15.764 <i>1.40</i>
Year 1986	25.197** <i>2.36</i>	25.498** <i>2.35</i>	22.739** <i>2.12</i>
Year 1987	21.496** <i>2.11</i>	21.439** <i>2.10</i>	17.676* <i>1.71</i>
One Co-Author	9.926 <i>1.42</i>	10.073 <i>1.43</i>	9.885 <i>1.43</i>
Two Co-Authors	0.295 <i>0.02</i>	0.367 <i>0.02</i>	2.056 <i>0.13</i>
Three or more Co-Authors	57.127 <i>1.10</i>	56.826 <i>1.09</i>	55.483 <i>1.07</i>
JEL Code Variables Included	No	No	No
N	576	576	576
R-squared	0.235	0.235	0.242

Notes: Coefficients in bold, *t*-statistics in italics. To obtain robust standard errors in these estimations, we use the Huber/White/Sandwich estimators of standard errors. The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. Citations accumulated up until September 2008.

TABLE 14B: CONNECTIONS AND CITATIONS SUCCESS

Independent Variables:	Dependent Variable: Total Citations			
	[4]	[5]	[6]	[7]
University Connection (Current Affiliations)	-3.584 <i>-0.21</i>		-3.39 <i>-0.19</i>	
University Connection (Past Affiliations)		0.818 <i>0.06</i>		1.825 <i>0.13</i>
PhD University Connection	21.339* <i>1.77</i>	21.044* <i>1.73</i>	22.67* <i>1.90</i>	22.266* <i>1.86</i>
Length	4.106*** <i>2.63</i>	4.058*** <i>2.57</i>	4.041*** <i>2.71</i>	3.973*** <i>2.63</i>
Main Article (Dummy)	19.292** <i>2.55</i>	19.142** <i>2.38</i>	21.479** <i>2.52</i>	21.273** <i>2.41</i>
First Article (Dummy)	-29.003* <i>-1.82</i>	-29.105* <i>-1.82</i>	-21.061 <i>-1.43</i>	-21.154 <i>-1.43</i>
Female Share	1.505 <i>0.17</i>	1.593 <i>0.18</i>	3.492 <i>0.38</i>	3.59 <i>0.39</i>
Academic Age	-0.496 <i>-1.03</i>	-0.473 <i>-0.96</i>	-0.668 <i>-1.44</i>	-0.645 <i>-1.36</i>
Authors Reputation	5.551 <i>1.59</i>	5.41 <i>1.50</i>	5.251 <i>1.55</i>	5.089 <i>1.47</i>
Equations Main Text	-0.429 <i>-1.40</i>	-0.42 <i>-1.35</i>	-0.32 <i>-1.11</i>	-0.308 <i>-1.06</i>
Tables Main Text	-0.216 <i>-0.09</i>	-0.185 <i>-0.08</i>	0.367 <i>0.16</i>	0.405 <i>0.18</i>
Figures Main Text	6.591* <i>1.75</i>	6.57* <i>1.73</i>	6.252* <i>1.65</i>	6.25 <i>1.63</i>
References per Article	0.763 <i>1.33</i>	0.773 <i>1.34</i>	0.814 <i>1.46</i>	0.83 <i>1.47</i>
Year 1984	13.009 <i>1.10</i>	13.332 <i>1.13</i>	16.371 <i>1.39</i>	16.852 <i>1.42</i>
Year 1985	15.886 <i>1.44</i>	15.853 <i>1.33</i>	21.436* <i>1.89</i>	21.437* <i>2.73</i>
Year 1986	22.751** <i>2.12</i>	22.832** <i>2.09</i>	28.673*** <i>2.66</i>	28.81*** <i>2.61</i>
Year 1987	17.795* <i>1.72</i>	17.689* <i>1.70</i>	21.388** <i>2.02</i>	21.234** <i>1.99</i>
One Co-Author	9.845 <i>1.42</i>	9.921 <i>1.42</i>	11.422 <i>1.61</i>	11.546 <i>1.62</i>
Two Co-Authors	2.003 <i>0.13</i>	2.062 <i>0.13</i>	-1.387 <i>-0.08</i>	-1.39 <i>-0.08</i>
Three or more Co-Authors	55.90 <i>1.08</i>	55.449 <i>1.07</i>	57.408 <i>1.13</i>	56.845 <i>1.12</i>
JEL Code Variable Included	No	No	Yes	Yes
N	576	576	576	576
R-squared	0.242	0.242	0.284	0.284

Notes: Coefficients in bold, *t*-statistics in italics. To obtain robust standard errors in these estimations, we use the Huber/White/Sandwich estimators of standard errors. The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. Citations accumulated up until September 2008.

According to our findings, gender has no impact on future citation success. Academic age is also negative (in line with Laband and Piette's findings on their author's mean age variable), but in our case, it is not statistically significant. We also find that the number of coauthors has no statistically significant impact on future citation success.⁸² This latter observation raises the questions already asked by Hollis (2001: 521–522): “Why would such a relationship exist? (And why do economists persist in working together, if indeed there is a negative effect on output per author?)” Based on his empirical results, Hollis suggests “three types of explanations for the apparent negative relationship between output and collaboration. First, there is the possibility of systematic mismeasurement. The second type of explanation assumes teamwork is chosen for some reason other than its effect on output, which may lead to unproductive collaboration. The third class of explanation assumes that teamwork is *endogenously* determined and that the observed relationship is the result of the individual choosing to be sole author of the ‘easiest’ and most productive projects.”

As regards the remaining control variables, we observe a positive relationship between the number of figures in a paper and the citations; however, the coefficient is not statistically significant in one of the seven specifications. Other article characteristics, such as number of equations, tables, or references per article, do not seem to matter. In addition, the year dummies indicate that “having more time” may not per se result in more publications. We

⁸² Hollis (2001: 521) explores in detail the relationship between coauthorship and the output of academic economists using citation data for the years 1995 to 2000 and a subsample of 74 authors whose surname starts with A. He then regresses the number of citations on the number of authors and a year index, using dummies to control for author effects. He not only shows that on average, an extra coauthor increases the number of citations by approximately two from a mean of six, but that a higher average rate of coauthorship increases the frequency of publishing articles. Taking this latter into account, he then calculates at the mean that, when the number of authors increases by one, the number of citations per author declines by around 10–20%. He thus concludes that the “fact that all of the methods I used to deal with zero-output problem resulted in a single conclusion—that co-authorship is negatively related to the current output of academic economists—demands an explanation. The results presented above show that for most economists, while collaboration appears to increase the frequency, quality and length of publication, it is also correlated with lower total output per author after discounting for the number of authors” (521).

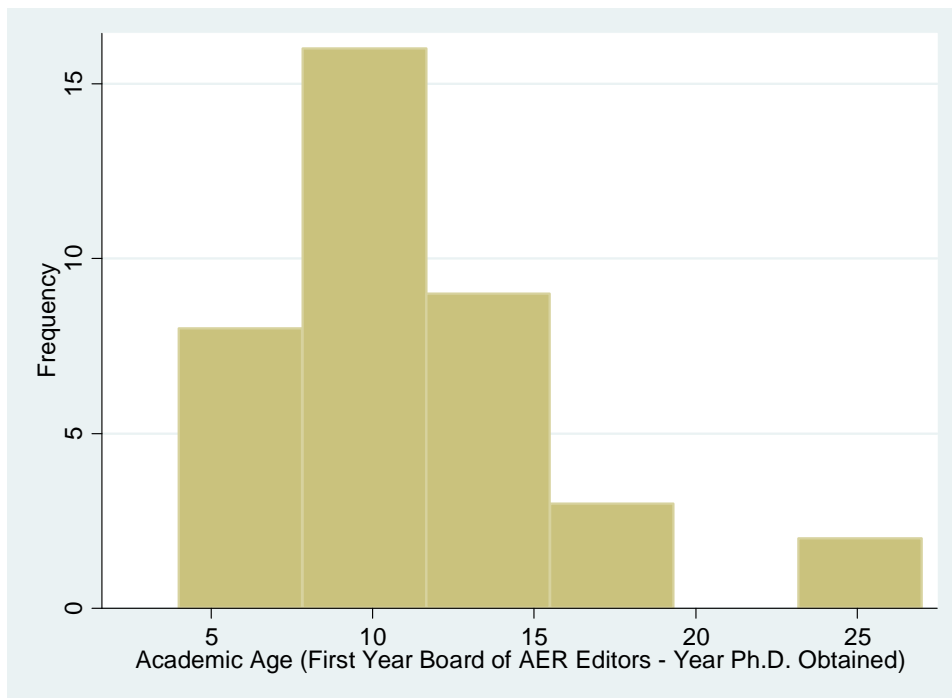
also conduct several robustness tests. For example, assuming publication delay to be a possible source of bias in our connection variables (i.e., the influence of an editor who leaves a journal in 1984 may still be visible in publications appearing in 1985) and taking into account that there have been editorial changes throughout the period under investigation, we report (in the appendix) a set of estimations that allow for a publication delay of one year. As evidenced in tables A4a and A4b, the previous results remain robust.

5.4 Board of AER Editors

Between 1911 and 1951, six people were part of the *AER* editorial board, whose members are usually relatively active in reviewing papers. In fact, Coats (1969) claims that in the 1940s, Homan's ability to command the respect of his fellow economists contributed to his success because, in addition to a willingness to submit manuscripts and write reviews, he was able to enlist the services of some of "the most gifted younger economists' as members of his editorial board" (p. 63). In 1956, the size of the editorial board increased to 7 and then to 8 in 1958. It increased again during the Gurley editorship (1963–1968) to 10 members and today (2011) includes 40 economists.⁸³

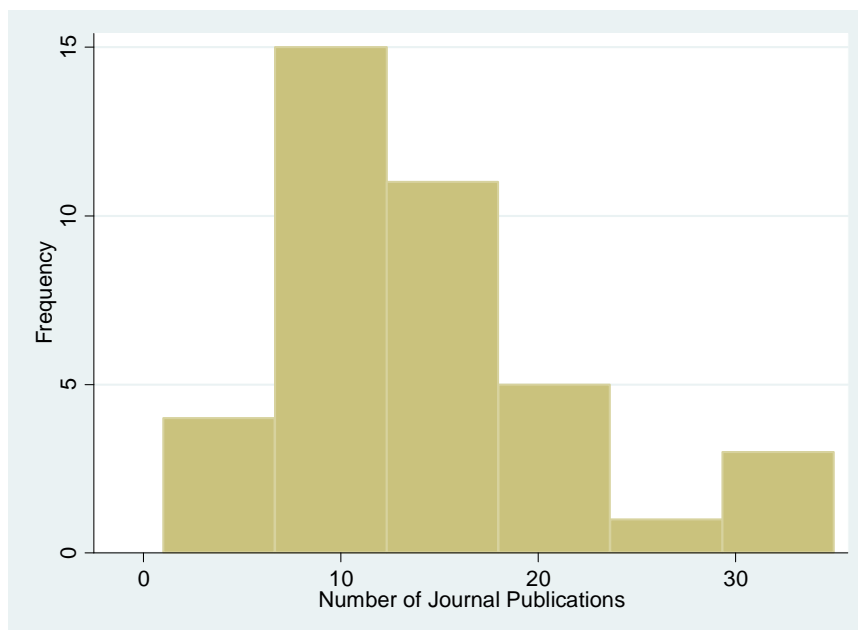
⁸³ See table A5 (<http://www.aeaweb.org/aer/index.php>).

FIGURE 13: ACADEMIC AGE OF *AER* BOARD MEMBERS IN JANUARY 2011



Notes: Board members taken from <http://www.aeaweb.org/aer/index.php>, accessed January 2011.

FIGURE 14: NUMBER OF JOURNAL PUBLICATIONS OF *AER* BOARD MEMBERS LISTED IN JANUARY 2011



Notes: Board members taken from <http://www.aeaweb.org/aer/index.php>, accessed January 2011.

For the analysis of board member profiles, we look in detail only at the *AER* Board of Editors as it appeared in January 2011 (40 individuals).⁸⁴ Seventeen board members are in the top 5% of the IDEAS author ranking, 25 are in the top 10%, and 30 out of 40 are affiliated with the universities listed in table 1. An examination of where these individuals obtained their Ph.D.s shows that MIT leads with 9 researchers, followed by Harvard with 5, and Chicago and Stanford with 4. These figures are also very consistent with tables 1 and 5. An overview of the board members' academic ages is provided in figure 13. On average, they have almost 10 years of academic experience post doctorate, and, interestingly, are significantly younger than the coeditors ($z = 3.965$), which may partly reflect a strategy comparable to Homan's of attracting gifted younger economists to the board. The distribution of board member journal publications, given in figure 14, shows that when they start their term, they have on average 14 journal publications, a significantly lower number than the coeditors ($z = 5.624$). Nonetheless, when *Papers and Proceedings* are included, most board members had published in *AER* when they joined the board.⁸⁵ Only 12.4% had never published in *AER*, although 40% had just one *AER* publication. Twenty percent had no publication in the other top journals *QJE*, *JPE*, and *Econometrica*, and 37.5% had only one publication in these journals.

6. *Submission Strategies, Referees, and Awards*

6.1 *Submission Strategies and Submitted Papers*

In a paper published in *AER*, Sharon M. Oster (1980: 444) asks an important question: "To which of the many available journals should a paper be sent?" To address this question,

⁸⁴ In some cases, we had to exclude Miguel Costa-Gomes and Holger Sieg because of the limited availability of online data on these two board members.

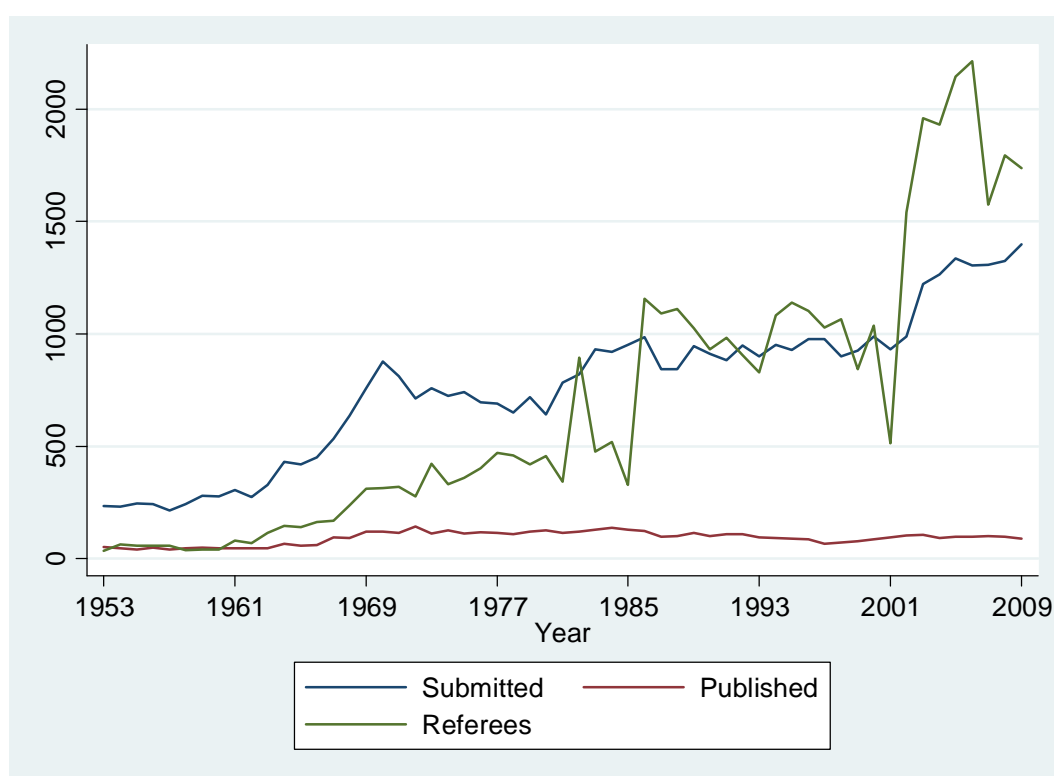
⁸⁵ Once *Papers and Proceedings* are excluded, 80% of the board members have previously published in *AER* (47.5% once).

she develops two alternative objective functions: “journal choice based on maximizing the stream of ‘prestige’ points from an article versus journal choice based on maximizing the discounted stream of readers of the article.” She defines the choice process as follows: “For each journal i , there is some probability, P_i , that the paper will be accepted. If it is accepted, the article earns its author a discounted stream of benefits over his or her lifetime. If the article is rejected by the first journal, it can then be submitted to a second journal, or a third. If there were no costs to a rejection, one would always adopt the strategy of first submission to the ‘best’ journal. As it is, there is a cost to being rejected (other than the obvious psychic one): the rejection process takes time and while one is waiting the articles obsolesces, tenure slots fill up with other people, and so on. In short, one trades off waiting against the quality of journal that finally accepts the article.” She therefore develops a formal optimization problem.⁸⁶ For this decision process, she focuses on eight journals: *AER*, *Econometrica*, *Economic Inquiry*, *International Economic Review*, *Southern Economic Journal*, *Journal of Political Economy*, *Quarterly Journal of Economics* and *Review of Economics and Statistics*. Her results, presented in appendix table A6, indicate that for a professor or assistant professor seeking prestige, the submission choice number 1 would be *AER*. *AER* should also be the first choice for a patient assistant professor seeking readers. It would be the second choice, however, for a professor or assistant professor seeking readers, for an impatient assistant professor, or finally, for a prestige-seeking professor nearing retirement.

⁸⁶ $B = P_1 \int_{w_1}^T g_1 e^{-rt} dt + (1 - P_1) P_2 \int_{w_1+w_2}^T g_2 e^{-rt} dt, \dots, + P_n \prod_{i=1}^{n-1} (1 - P_i) \int_{\sum_{i=1}^n w_i}^T g_n e^{-rt} dt$, where B = expected benefits from submitting to journals in the order 1,2,3... n , $P_1 \dots P_n$ = the probability of acceptance in journal 1... n (for *AER* = 0.14, although, as the entry for 2009 in figure 12 shows, today, it would be lower), $g_1 \dots g_n$ = gain from acceptance in journal 1... n measured through number of prestige points based on survey study among 160 economists (as an alternative f , the size of the journals’ readership; highest values for *AER*), $w_1 \dots w_n$ = waiting time before decision by journal 1... n (0.33 for *AER*), and r = discount rate (see appendix table A6). For T , she assumes retirement age (no posthuman glory), and P s are independent of each other (e.g., rejection by *AER* does not itself diminish the probability of acceptance in another journal). The effect of time loss is incorporated in the discount rate and not through probabilities.

The number of papers submitted to *AER* has increased substantially over time, which has led to an increase in rejections (see figure 15). In 1953, 22% of the papers submitted were published, whereas in 2009 the number fell to only 6.4%. This increasing number of submissions also increases the editorial burden, which requires a large referee pool. Because the annual Report of the Editor has also provided the names of *AER* referees (since 1953), we were able to count the yearly numbers and graph them between 1953 and 2009 (see figure 15). This figure does indeed show that the number of referees has significantly increased over time and quite substantially in the last 10 years.

FIGURE 15: SUBMITTED AND PUBLISHED PAPERS AND REFEREES IN *AER* (1953–2009)



We also attempt to get an idea of the sort of papers that have been accepted or rejected. For the period during which George H. Borts was editor, the editorial reports not only provide general data on the submission and acceptance rate over time but also the division of topics. Appendix table A7 summarizes this information for 1969 to 1980 and, although it

shows an increase in rejections over time, reveals no clear rejection pattern throughout that period. On the contrary, we observe a high level of volatility, in particular for those subject areas in which fewer papers were submitted. Such significant differences across years and among the different subject areas may indicate a fair review process or at least a process that is not driven by editorial preferences based on subject areas.

6.2 Referees

Although referees are important for a journal's success, they may not necessarily act in the best interest of science as a whole (Frey 2005). First, since they have no property rights to the journal, they may not be concerned with the effect that their advice has on the publication. This absence of property rights could also lead to shirking behavior. Moreover, because a referee report has minor consequences for the referee (a low-cost situation), it may be tempting for referees to judge submitted papers based on whether their contributions are sufficiently appreciated and cited. They may even reject a paper based on a dislike that is subjectively driven (e.g., if their own work is criticized in the paper). In fact, based on information gathered from 50 referee requests⁸⁷ sent to seven editors, Hamermesh (1994: 155) finds that referees "are neither neophytes nor gray-beards. The mean Ph.D. experience is 16 years, implying that the average referee is roughly 45. Not surprisingly, people are asked to referee increasingly as they near the peak of their careers. Past that peak, they are called on at a diminishing rate." His data set also shows that 95% of referees are men and nearly one-third had published recently in the requesting journal (others had articles under review or forthcoming at the time they were asked to act as referee). In addition, "with an entire profession to choose from—there are economists in over 2000 institutions of higher education in the United States alone—the extent that editors rely on colleagues in their own

⁸⁷ Out of 350 possible data points, 343 were usable.

departments attests to the role of propinquity in the choice of referees” (156). In summarizing his results, he notes that “referees are disproportionately the top people in their specialty. But editors also rely heavily on scholars to whom they have easy access” (156).

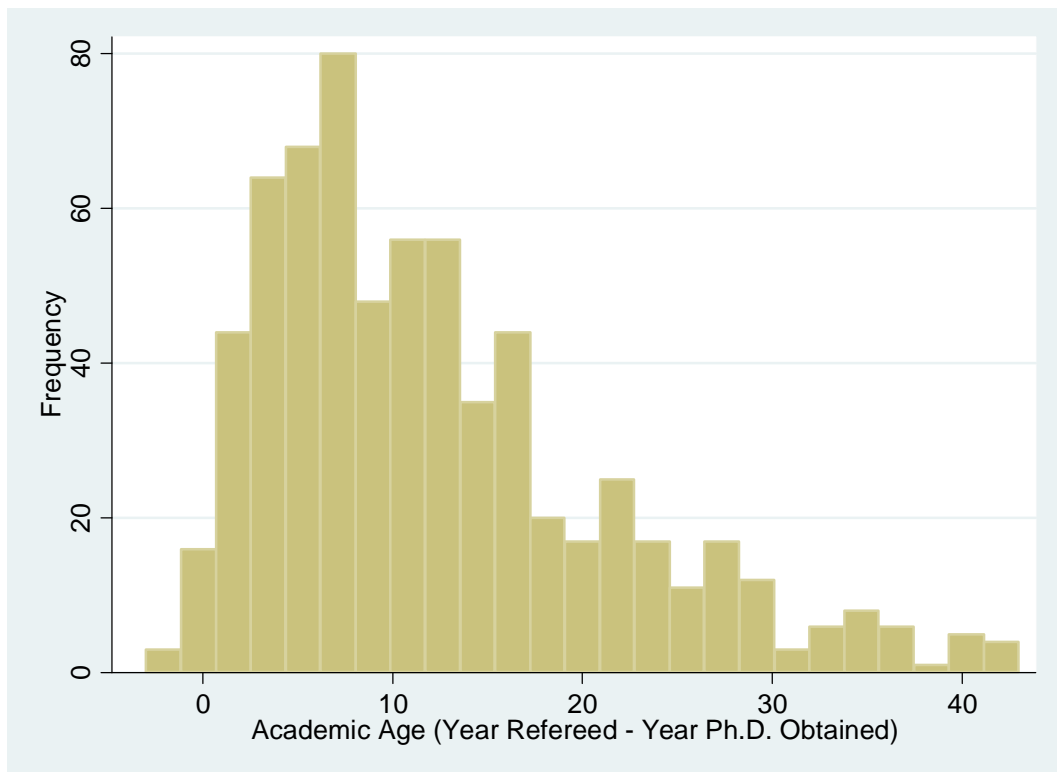
To conduct a similar analysis, we collected data on recent *AER* referees. The sample consists of 676 referees for the periods 2007, 2008, and 2009. We took the data from the Report of the Editor published in the 2009 and 2010 *Papers and Proceedings*, which announced in 2009 a new award titled the *AER* Excellence in Refereeing Awards “for referees who have provided exceptional services to the *Review* by a large number and quality of referee reports” (664). In 2008, 47 individuals were recipients, while in 2007, 79 received the award. The 2010 Report of the Editor names 212 award-winning individuals, meaning that a total of 338 awards have been distributed. We then randomly selected the same number of names per year from the nonaward winning referees (79, 47, and 212), giving us a total of 158 observations for 2007, 94 for 2008, and 424 for 2009. Next, we collected background data on these referees via the Web (including details such as institutional affiliations, university at which they obtained their Ph.D., gender, academic age (year of refereeing for *AER* – year Ph.D. obtained), publication record in top journals (number of *AER*, *QJE*, *JPE*, and *Econometrica* publications at the time of refereeing for *AER*), and position internationally on the IDEAS ranking).⁸⁸ Of the 207 different institutions from which these referees come, the 10 with the highest referee participation are (in descending order) New York University (4.74%), Columbia University (4%), Harvard University (3.56%), Stanford University (3.11%), University of California, Berkeley (2.96%), University of Chicago (2.96%), Northwestern University (2.67%), University of Michigan (2.37%), Yale University (2.22%), and University of Pennsylvania (2.22%). Together, these institutions are responsible for 30.8% of the refereeing work carried out. When we examine this same factor looking at

⁸⁸ Average ranking score as of November 2010; for the latest update, see <http://ideas.repec.org/top/top.person.all.html>

the referees' Ph.D. affiliations, we note significantly fewer institutions, 121 rather than the 207 reported beforehand. Of these, the following 10 universities have the largest share of referee engagement: Harvard University (10.55%), MIT (9.36%), Stanford University (6.69%), University of Chicago (5.65%), Princeton University (4.75%), Yale University (3.86%), Northwestern University (3.57%), University of California, Berkeley (3.57%), University of Pennsylvania (3.12%), and University of Wisconsin (3.12%). Thus, these 10 universities are responsible for 54.24% of the refereeing work carried, reflecting a substantially increased concentration that is consistent with the ranking evaluations presented at the beginning of our paper. Moreover, the universities listed here are the same institutions that appear in the list of the 10 best universities in table 1 with only one difference, the University of Wisconsin instead of the University of California, Los Angeles. Overall, the ordering based on the share of refereeing is not far from the ranking structure reported in table 1, and our referee statistics are similar to the rankings given in table 5: 8 out of 10 institutions are the same and the best 5 universities are identical. Nonetheless, there is less concentration in the referee market.

Of these referees, 14.34% were female, a slightly larger proportion than the 12.24% of female researchers who published primary articles in *AER* between 2001 and 2010. On average, referees have an academic age of 12 (see figure 16 for the academic age distribution), and 62% have an academic age of 12 and below. Only 20% of referees have an academic age of 19 years and above, so our group of referees is relatively younger than those in Hamermesh's (1994) dataset. In terms of IDEAS ranking, our data indicate that 12% have been positioned among the top 5%. However, only 1.42% of the referees had previously published in *AER* (including *Papers and Proceedings*) and only 1.28% in *QJE*, *JPE*, or *Econometrica*.

FIGURE 16: DISTRIBUTION OF REFEREES' ACADEMIC AGE



6.3 Awards

Bruno A. Frey (2006b: 377) once remarked that “[i]f an alien were to look at the social life of people here on earth, it would be stunned by the enormous number of awards in the form of orders, medals, decorations, prizes, titles, and other honours. It would be hard pressed to find any area of society in which awards are not used.” Thus, it is not surprising that universities and the academic environment in general have also developed an extensive system of awards (Bruno S. Frey and Susanne Neckermann, 2009). Indeed, Bruno S. Frey and Margit Osterloh (2010:871) note that the “incentive system for scholars has to match their main motivation factors. Prizes and titles are better suited for that purpose than citation metrics. Honorary doctorates, different kinds of professorships and fellowships (from assistant to distinguished), membership of scientific academies and honours such as the Fields Medal or Nobel prizes are great motivation even for those who do not actually win

such a prize. The money attached to such rewards is a bonus, but less important than the reputation of the award-giving institution.” Compared to monetary payments, such accolades are “less likely to destroy the signal value of actions requiring special commitment, or of actions beyond what is typically expected” and are motivating because winning an award “makes the recipient feel good about himself or herself” (Frey and Neckermann 2009: 76). Awards are provided by a “principal whose opinion the agent values” and generate “social prestige and bring recognition within the peer group”; they are also “typically set up as tournaments and many persons enjoy competing; that is, working towards an award generates process utility.” Thus, the recent *AER* policy of rewarding a selection of those who have volunteered their services may generate a positive motivational effect. As an indication that researchers take this award seriously, we note that some recipients have listed the *AER* award on their CV. However, the contingent of referees who have earned this *AER* award is large, and indeed some in our data set have already received it twice,⁸⁹ which may reduce its attractiveness. That is, awards “are in demand only if they are valuable to recipients because they signal distinction to other persons. If an award is easy to get, it loses this value and no longer serves this purpose” (Frey 2006b: 381)

Frey and Neckermann (2009: 74) also point out that despite “the importance of awards in society, economists have largely disregarded them.” There are, however, very few empirical studies on this area of economics and awards, a shortfall we attempt to rectify.⁹⁰ We first

⁸⁹ The following individuals in our dataset received the award twice: Ashish Arora, Lori Benneer, Marianne Bertrand, Nicholas Bloom, Ariel Tomas Burstein, Luis Cabral, Estelle Cantillon, Eugene Choo, Brian Copeland, Martin Dufwenberg, Raymond Fisman, Michael Grubb, Peter Ireland, Emir Kamenica, Kai A. Konrad, Marco Mariotti, Virgiliu Midrigan, Massimo Morelli, Benjamin Olken, Jesse Shapiro, Matthew Shapiro, Dan Silverman, Joel Waldfogel, and Stephen Yeaple.

⁹⁰ For a recent empirical paper on awards, see, for example, Susanne Neckermann, Reto Cueni, and Bruno S. Frey (2009). These authors use an interesting employee performance dataset for 155 call center agents of a credit card service company over the January 2004 to October 2007 period to explore a so-called Gold Reward given for exceptional efforts that benefit the entire work group. Their performance index comprises a large variety of performance factors (calls taken per hour, call handling time, after call work time, transfer rate to colleagues or other service units, days employee shows up late, and client-based quality assessment), most of which are evaluated in relative terms (scaling based on percentage deviation between individual performance and the average monthly performance of all call center agents). Their results indicate that award winners

investigate whether there are characteristics differences between *AER* referees who received awards and those who did not. Our results show that award winning referees are academically younger than the other referees (11.91 vs. 12.52), less likely to be female (11.8% vs. 16.9%), and slightly more likely to have *AER* publications (1.6% vs. 1.25%) or publications in the top journals such as *QJE*, *JPE*, or *Econometrica* (1.4% vs. 1.08%). They are also more likely to be affiliated with the 10 best universities reported in table 1 (30% vs. 20.7%) or to have a Ph.D. from the 10 best universities (58% vs. 47%). The award-winning referees are also more likely to be in the top 5% of the IDEAS ranking⁹¹ (28.9% vs. 19.8%).

In table 15, we present the estimates from probit regressions in which the dependent variable is whether someone received the award (= 1) or not. We present various specifications that apply different quality measures (e.g., referee's affiliation or referee's Ph.D. institution). In the last two specifications, we exclude the researchers who appear twice in our data set. We find that being female rather than male reduces the probability of receiving the *AER* Excellence in Refereeing Award by over 10%. Academic age is also negatively correlated with the probability of earning an award: a marginal change in academic age of one year (from the average 12) reduces the probability of receiving an award by close to 1%. The quality of the referees also seems to make a significant difference. Being ranked in the top 5% of researchers worldwide increases the probability of receiving an award by around 15%. Similarly, affiliated referees in the best 10 universities listed in table 1 have a 9% higher probability of being award recipients, and holding a Ph.D. from these universities also increases the award probability by at least 8%. Extending the number of institutions to the entire list reported in table 1 leads to similar results. For affiliated referees in these institutions we even observe a 15% higher probability of being awarded. The number of accumulated *AER* publications (including *Papers and Proceedings* articles) before

increase their subsequent performance relative to their previous performance and relative to nonaward employees.

⁹¹ IDEAS ranking as of November 2010.

refereeing⁹² for *AER* is positively correlated with award reception, but the coefficient is not statistically significant in all specifications.

Next, we explore whether earning an award for refereeing in 2007 has an impact on publishing a primary article in 2009 focusing only of those who refereed in 2007⁹³. Specifically, we explore the number of primary articles published in *AER* during 2009 reporting probit estimates (dependent variable = whether or not a referee has published in *AER*). Controlling for referee characteristics such as gender, academic age, and more important, quality,⁹⁴ we find evidence that receiving an award has positive publications externalities. Indeed, when factors like referee quality are held constant, the probit estimates show that obtaining an award for refereeing in 2007 increases the probability of publishing a primary article in 2009 by around 10% (statistically significant at the 1% level in all two estimations). We also find that referees who obtained a Ph.D. from the best 10 universities (see table 1) have a higher probability of publishing in *AER* in 2009. On the other hand, referees who rank in the top 5% of the IDEAS ranking are not more likely to publish in *AER* (coefficient is not statistically significant). Similarly, the coefficient for the number of previous *AER* publications achieved up to 2007 is not statistically significant.⁹⁵

⁹² One year beforehand.

⁹³ One should note that we have no information on whether these referees actually submitted a paper to *AER*.

⁹⁴ Affiliation and Ph.D. in the best 10 universities listed in table 1 and being in the top 5% of researchers worldwide according to the IDEAS ranking.

⁹⁵ Although we do not report the results here, we also explore the number of *AER* publications (with and without *Papers and Proceedings*): the coefficient was positive but not statistically significant.

TABLE 15: DETERMINANTS OF OBTAINING THE *AER* EXCELLENCE IN REFEREEING AWARD

Probit Model, Dependent Variable: Award (yes = 1, no = 0)						
Independent Variables	[1]	[2]	[3]	[4]	[5]	[6]
Female	-0.281** (-1.97) <i>-0.111</i>	-0.299** (-2.08) <i>-0.118</i>	-0.279** (-1.96) <i>-0.110</i>	-0.273* (-1.9) <i>-0.108</i>	-0.285* (-1.92) <i>-0.111</i>	-0.300** (-2.01) <i>-0.117</i>
Academic Age	-0.021*** (-2.88) <i>-0.008</i>	-0.022*** (-3.03) <i>-0.009</i>	-0.023*** (-3.26) <i>-0.009</i>	-0.019*** (-2.69) <i>-0.008</i>	-0.018** (-2.47) <i>-0.007</i>	-0.017** (-2.32) <i>-0.007</i>
Referee Affiliated with Best 10 Institutions (Table 1)	0.224* (1.89) <i>0.089</i>					
Referee Ph.D. from one of the Best 10 Institutions (Table 1)		0.247** (2.45) <i>0.098</i>				0.184* (1.76) <i>0.073</i>
Ph.D. from Institution in Table 1			0.219** (1.98) <i>0.087</i>		0.191* (1.68) <i>0.075</i>	
Affiliated with Institution in Table 1				0.371*** (3.63) <i>0.147</i>		
IDEAS Top 5%	0.386*** (2.92) <i>0.152</i>	0.388*** (2.94) <i>0.153</i>	0.383*** (2.90) <i>0.151</i>	0.394*** (2.97) <i>0.156</i>	0.362*** (2.63) <i>0.144</i>	0.365*** (2.66) <i>0.145</i>
Number <i>AER</i> Publications until Refereeing	0.052* (1.78) <i>0.021</i>	0.054* (1.88) <i>0.021</i>	0.059** (2.06) <i>0.023</i>	0.042 (1.42) <i>0.017</i>	0.044 (1.48) <i>0.017</i>	0.042 (1.39) <i>0.016</i>
<i>N</i>	666	666	666	666	619	619
Prob.>chi ²	0.000	0.000	0.000	0.000	0.001	0.001
Pseudo <i>R</i> ²	0.029	0.032	0.030	0.040	0.023	0.024

Notes: Coefficients in bold, z-statistics in parentheses, marginal effects in italics. The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 16: RELATIONSHIP BETWEEN RECEIVING AN AWARD IN 2007 AND PUBLISHING IN *AER* IN 2009

Probit Regressions, Dependent Variable: <i>AER</i> Publication in (2009 yes=1, 0=no). Researchers who refereed in 2007.		
Independent Variables	[1]	[2]
Award 07	0.879** (2.36) <i>0.114</i>	0.902** (2.35) <i>0.098</i>
Academic Age	-0.045 (-1.49) <i>-0.006</i>	-0.051* (-1.66) <i>-0.005</i>
Female	0.088 (0.20) <i>0.011</i>	-0.032 (-0.07) <i>-0.003</i>
Number <i>AER</i> Publications (without <i>Papers and Proceedings</i> Publications) up to 2007	0.251 (1.42) <i>0.031</i>	0.221 (1.23) <i>0.022</i>
Referee Affiliated in Best 10 Institutions (Table 1)	-0.042 (-0.11) <i>-0.005</i>	
Referee Ph.D. Obtained in Best 10 Institutions (Table 1)		0.761** (2.08) <i>0.078</i>
IDEAS Top 5%	-0.005 (-0.01) <i>0.001</i>	0.163 (0.39) <i>0.018</i>
N	155	155
Prob>chi2	0.029	0.004
Pseudo R2	0.143	0.192

Notes: Coefficients in bold, t-statistics in parentheses, and marginal effects in italics. The symbols *, **, *** represent statistical significance at the 10%, 5% and 1% levels, respectively.

7. Paper Characteristics

Finally, we take a closer look at the characteristics of papers published in *AER*, with a focus on length, subject content, and a third measure that is not free of controversies, level of technique.

7.1 Length

The “length inflation” of *AER* papers has been discussed by Margo (2011),⁹⁶ who finds that the length increased from an average 17.2 pages in 1950 to 22 pages in 2005. There were, however, years in between in which the articles were substantially shorter; for example, in 1970, the average length of a paper was 12.6 pages and in 1980 it was 13.1. Thirteen years before Margo’s study, David N. Laband and John M. Wells (1998) published a paper in *American Economists* that explores the changing length of articles over time. Their focus was on the general-interest journals *AER*, *JPE*, and *QJE* from their respective beginnings until 1995. They show that prior to 1970, there was considerable yearly volatility in the average length of a journal article, trending toward a decline in the average article length from around 23 to 17 pages. However, since 1970, there has been less volatility and a relatively rapid increase in length, with the average again reaching 23 pages. The authors also present a regression that explores the impact of JEL subject codes⁹⁷ on the page length of feature articles without controlling for further factors. The constant shows a historical mean length of 19.45 pages. In general, articles on general economic theory and international economics are significantly shorter than those on other subjects, whereas papers on business administration; industrial organization, technical change, and industry studies; and quantitative methods and data are nearly 10% longer on average than papers on general economic theory.

⁹⁶ See table 2, page 47.

⁹⁷ They applied a previous 10-category JEL code system.

TABLE 17: DETERMINANTS OF PAPER LENGTH

Dep. Var.: Length of Main Article	[1] Full Model	Estimated Length	[2] Main Articles Only	Estimated Length
(A) General Economics and Teaching	-1.588 <i>-1.02</i>	6.43	-2.585 <i>-1.29</i>	8.13
(B) Schools of Economic Thought and Methodology	0.796 <i>0.64</i>	8.82	1.747 <i>1.07</i>	12.46
(C) Mathematical and Quantitative Methods	1.114 <i>1.24</i>	9.14	1.203 <i>1.37</i>	11.92
(D) Microeconomics	0.362 <i>0.71</i>	8.38	-0.036 <i>-0.07</i>	10.68
(E) Macroeconomics and Monetary Economics	2.238*** <i>3.56</i>	10.26	1.676*** <i>2.66</i>	12.39
(F) International Economics	1.768** <i>2.31</i>	9.79	1.164 <i>1.55</i>	11.88
(G) Financial Economics	2.735*** <i>3.34</i>	10.76	1.752** <i>2.12</i>	12.47
(H) Public Economics	0.180 <i>0.27</i>	8.20	0.847 <i>1.14</i>	11.56
(I) Health, Education, and Welfare	0.785 <i>0.82</i>	8.81	-0.256 <i>-0.25</i>	10.46
(J) Labor and Demographic Economics	2.215*** <i>3.77</i>	10.24	1.931*** <i>3.18</i>	12.65
(K) Law and Economics	1.063 <i>0.74</i>	9.09	-0.684 <i>-0.48</i>	10.03
(L) Industrial Organization	1.220* <i>1.92</i>	9.24	1.064 <i>1.59</i>	11.78
(M) Business Administration and Business Economics; Marketing; Accounting	0.223 <i>0.17</i>	8.24	-0.225 <i>-0.14</i>	10.49
(N) Economic History	2.683** <i>2.19</i>	10.71	1.829 <i>1.49</i>	12.55
(O) Economic Development, Technological Change, and Growth	1.100 <i>1.35</i>	9.12	1.563* <i>1.90</i>	12.28
(P) Economic Systems	1.664 <i>1.23</i>	9.69	1.758 <i>1.26</i>	12.48
(Q) Agricultural, Natural Resources; Environmental, Ecological Economics	0.242 <i>0.21</i>	8.26	-1.017 <i>-0.82</i>	9.70
(R) Urban, Rural, and Regional Economics	0.829 <i>0.61</i>	8.85	0.803 <i>0.67</i>	11.52
(Z) Other Special Topics	1.621 <i>1.13</i>	9.64	0.253 <i>0.18</i>	10.97
2004–2008 Dummy	9.379*** <i>17.3</i>	17.40	8.508*** <i>15.73</i>	19.23
One Coauthor	0.434 <i>0.91</i>	8.46	0.989* <i>1.99</i>	11.71
Two Coauthors	1.360* <i>1.72</i>	9.38	1.819** <i>2.26</i>	12.54
Three or More Coauthors	0.339 <i>0.16</i>	8.36	-0.082 <i>-0.03</i>	10.64
Female Share	-0.042 <i>-0.04</i>	7.98	-0.542 <i>-0.49</i>	10.18
Academic Age	-0.068** <i>-2.52</i>	7.95	-0.078*** <i>-2.79</i>	10.64
N	1072		862	
R ²	0.343		0.330	

Notes: Coefficient in bold, *t*-statistics in italics. The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. In specification [2], we exclude shorter papers such as comments, replies, or notes. There is no reference group for subject codes as one paper can have more than one subject code.

Drawing inspiration from this study, we run OLS regressions on the relationship between JEL subject codes and article length but control also for gender (female share), academic age (year of publication – year Ph.D. obtained), the number of coauthors (using a dummy variable, with single-authored contributions as the control group), and changes over time (also using a dummy variable). Using the time periods 1984–1988 and 2004–2008, we present estimations both with and without short articles⁹⁸ but excluding *Papers and Proceedings*. As table 17 shows, similar to Laband and Wells (1998), we find that the subject-area general economics and teaching is characterized by the shortest articles, whereas labor and demographic economics, economic history, financial economics, and macroeconomics and monetary economics are the subject of longer papers. Thus, all these topics have a strong and significantly positive effect on the length of an *AER* paper. Looking at the control variables, we find that academic age is negatively correlated with article length, while gender does not seem to matter. *Ceteris paribus*, articles with three authors seem to be longer than single-authored papers (statistically significant at the 5% and 10% level); however, the other two dummies (one coauthor or three or more coauthors) are barely statistically significant. Interestingly, *ceteris paribus*, we find a strong increase in length between the 1984–1988 and 2004–2008 periods.

7. 2 Subject Areas

It is understandable that researchers have different ideas about the relative importance of particular subject areas, and we are unable to cover each of the different views. We therefore select two specific areas for illustrative discussion, the history of economic thought and economic history. As Coats (1969) documents, in the early period of the AEA (i.e., during

⁹⁸ We define short articles as notes, comments, replies, and so forth.

AER's pre-history),⁹⁹ the association came under criticism for becoming dependent on studies in economic history and economic thought. In 1895, H. H. Powers, then chairman of *AER*'s publication committee, stated that "we shall run the Association into the ground if we make it too much of an archaeological society" (57). Twenty years ago, William Baumol (1991), who has an extremely successful publishing record in *AER* (see table 9), authored a paper entitled "Toward a Newer Economics: The Future Lies Ahead!" in which he expresses rather sceptical sentiments about expanding the history of economic ideas: "[T]hough I have taught such a course for many years, I am much more sceptical about any attempt to inveigle more students in that direction. It is my belief that much attention is paid to the work of the past only in fields where there is currently little progress at the frontier.... Still, there are undoubtedly matters of greater urgency demanding the student's very scarce time, and so it is my predisposition to leave the area to those who are attracted to it (or to any other specialised research area) by what Veblen described as 'idle curiosity'" (5). In its stead, he favors the "reintroduction of emphasis on the teaching of economic history.... It seems to me that many institutional areas lend themselves to study via historical materials, and in some it may not even be possible to carry out effective research without them. Besides, for those whose forte is not a high level of abstraction, history is apt to prove a very good source of ideas and is apt to contribute considerably to general understanding. It should also provide vital practice in the empirical analysis of messy and complicated problems of which economic history has an endless supply" (5).

Table 18 provides an overview of how subject relevance has changed over time (specifically, between 1984–1988 and 2004–2008) based on the JEL codes reported in *AER*

⁹⁹ At this time, the association was putting out irregular series of monographs, conference proceedings, annual reports, and handbooks under the rubric Publications of the American Economic Association.

papers for 1,081 published articles (excluding *Papers and Proceedings*)¹⁰⁰. In the 584 articles published between 1984 and 1988, there were 1,490 JEL codes, a number that increased to 1,884 (497 articles) in the 2004–2008 period. Thus, it seems that on average, in recent years, authors have been using more JEL codes in their papers, possibly because doing so may help to increase the visibility of their papers in search systems such as *EconLit*. For both periods, we observe that “microeconomics” dominates the agenda, with a statistically significant increase from a 20% share in 1984–1988 to a 25% share in 2004–2008. In 1984–1988, “industrial organization” and “labor and demographic economics” had a slightly higher proportion of contributions than did “macroeconomics and monetary economics”; however, the shares for these two areas have decreased considerably over time at a highly statistically significant rate. Likewise, the share for “macroeconomics and monetary economics” has decreased from 12.28% to 10.08% (also statistically significant) and downward trends in the relative share are also observable for “public finance,” “general economics and teaching,” “history of economic thought, methodology, and heterodox approaches,” “international economics,” “business administration and business economics; marketing; accounting,” “economic systems,” and “agricultural and natural resource economics; environmental and ecological economics.” A relative increase in contributions is found, however, for “mathematical and quantitative economics,” “financial economics,” “health, education, and welfare,” “law and economics,” “economic history,” “economic development, technological change, and growth,” and “urban, rural, and regional economics.”

One driver of these developments might be the emergence in the 1970s and 1980s of highly specialized journals that influenced the relative importance of the subject areas published in *AER*. For example, in areas with a decreasing relative share, such top specialized

¹⁰⁰ We matched the JEL codes prior to 1991 to the current JEL codes based on an article published in *JEL* (“Classification System: Old and New Categories.” *Journal of Economic Literature*, 29(1), pp. xviii–xxviii).

journals emerged as the *Journal of Labor Economics* (founded in 1983), the *Journal of Monetary Economics* (1975), and the *Journal of Public Economics* (1972). Therefore, as Margo (2011: 26) points out, a “labor economist who came of age in 1960 could publish in the mainstream journals plus the *Industrial and Labor Relations Review* founded in 1947. Her counterpart twenty years later could add *Industrial Relations*, *The Journal of Human Resources* and *Research in Labor Economics* to the list. Twenty years after that the list has grown to include *The Journal of Labor Economics* and *Labour Economics*, and others.” Today, the number of economics journals is impressive: we counted a total of 1,484 journals listed in *EconLit*,¹⁰¹ and IDEAS includes information on 1,231 journals (including series).¹⁰² Thus, the subject areas showing an upward trend have also been affected by the emergence of such new publications as the *Journal of Financial Economics* (1974), the *Journal of Economic Theory* (1969), the *Journal of Econometrics* (1973), the *Journal of Development Economics* (1974), and the *Journal of Mathematical Economics* (1974). Nonetheless, given that other important journals—for example, the *International Economic Review* (1960) or the *Journal of Law and Economics* (1958)—were founded much earlier than the 1980s, it is not fully clear what might explain the changes in subject-area development over time.

In general, one does have to be careful in interpreting these results: disciplines can split in many different ways. For example, while there has been a growth in mathematical theory, there has been a decline in mathematics in empirical fields. Likewise, there has been growth in some fairly technical econometric applications, but in fields like labor and public economics, the increase in empirical work has been followed by a decline in purely econometric analysis.¹⁰³ Such arguments also hold for the next subsequent discussion in which we consider the technical level of the papers.

¹⁰¹ http://www.aeaweb.org/econlit/journal_list.php?full=false

¹⁰² <http://ideas.repec.org/>, accessed January 2011.

¹⁰³ We are thankful to Robert A. Moffitt for providing us these thoughts and facts.

TABLE 18: SUBJECT-MATTER DISTRIBUTION OF PAPERS OVER TIME

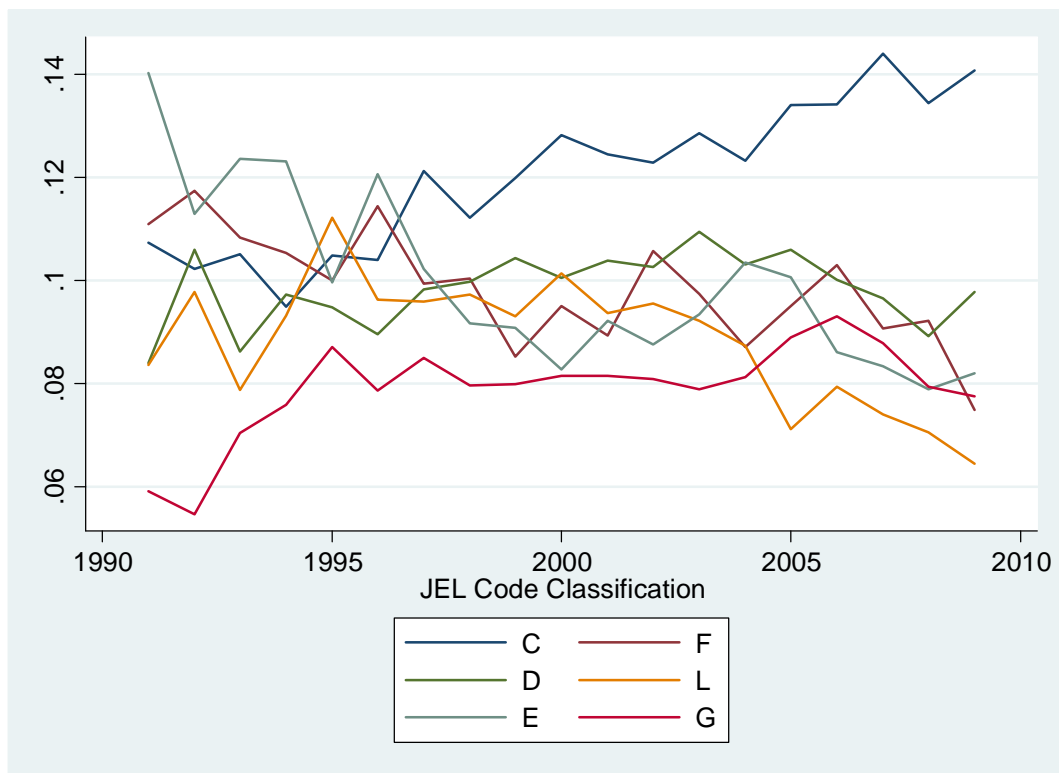
Variables	Period 1984 to 1988 N = 1490 (584 Articles)	Period 2004 to 2008 N = 1884 (497 Articles)	z - score
(A) – General Economics and Teaching	1.74%	0.42%	-3.873
(B) - History of Economic Thought, Methodology, and Heterodox Approaches	2.08%	0.16%	-5.196
(C) - Mathematical and Quantitative Methods	3.62%	6.53%	9.644
(D) - Microeconomics	20.07%	25.21%	20.905
(E) - Macroeconomics and Monetary Economics	12.28%	10.08%	-14.560
(F) - International Economics	7.38%	5.79%	-11.180
(G) - Financial Economics	3.96%	6.16%	9.849
(H) - Public Economics	8.32%	4.25%	-10.954
(I) - Health, Education, and Welfare	2.42%	5.10%	8.307
(J) - Labor and Demographic Economics	12.42%	8.97%	-14.731
(K) - Law and Economics	0.34%	2.49%	5.568
(L) - Industrial Organization	13.02%	8.12%	-14.177
(M) - Business Administration and Business Economics; Marketing; Accounting	1.95%	1.91%	-6.245
(N) - Economic History	1.01%	2.28%	5.916
(O) - Economic Development, Technological Change, and Growth	3.83%	6.37%	10.050
(P) - Economic Systems	1.54%	1.06%	-5.196
(Q) – Agricultural and Natural Resource Economics; Environmental and Ecological Economics	2.75%	1.17%	-5.745
(R) - Urban, Rural, and Regional Economics	1.28%	1.86%	5.477
(Y) Miscellaneous Categories	No observations		
(Z) Other Special Topics	0%	2.07%	-
Total	100%	100%	

To assess whether the relative importance of the subject areas in *AER* publications is also reflected in the relative priorities for the job market, we now compare the previous results with job market variables such as job listings and changes to the types of job openings listed. Although we must stress that the job market is more forward looking than journal publications, such data can give, in aggregated terms, some hints on the future development of economics departments and their long-run prioritization of research and teaching efforts. Therefore, using Job Openings for Economists (JOE) data for the last 20 years, we calculate the shares over time to measure the relative importance of the fields.¹⁰⁴ As figure 17 shows,

¹⁰⁴ Data collected from the Report of the Director: Job Openings for Economists, published in the *AER Papers and Proceedings*.

the top 5 fields in 1991 include “financial economics,” which was among this top group in 2009. Likewise, “microeconomics” (D) has remained relatively stable across the last 20 years, whereas the relative importance of “macroeconomics and monetary economics” (E), “international economics” (F), and “industrial organization” (L) have decreased. “Mathematical and quantitative methods” (B) and “financial economics” (G), on the other hand, show an upward trend.

FIGURE 17: TOP JOB OPENINGS IN PERCENT OVER TIME (1991–2009)



Notes: Shares relative to the total field. Data derived from the annual *Papers and Proceedings* (Report of the Director, JOE).

In appendix table A8, we also examine whether there is a correlation between the subject areas appearing in JOE and those appearing in *AER* publications during the same 1991 to 2009 period.¹⁰⁵ For 12 out of the 19 subject areas, we observe a positive correlation between job openings and the subject codes published, although the correlation is statistically

¹⁰⁵ *AER* publication data are taken from the Report of the Editor in various years.

significant in only 5 out of 19 cases (JEL codes C, E, L, N, R). These results may indicate differences between what is of interest to *AER* and the existing demand in the job market. It is clear, however, that these results should be viewed with caution. Not only is it problematic to take *AER* as representative of the entire economics discipline, but 19 years of data and a simple correlation analysis cannot provide insight into whether a job market develops hand-in-hand with the publication market. Rather, a dynamic analysis would provide additional valuable information.

7.3 Level of Technique

The level of economic theory and the use of mathematics in economics, which we here group under the broader label “level of technique,” has been a matter of discussion among economists for some time. *AER* has been no exception, and its past issues are scattered with transcripts of debates on the topic. For instance, during his testimonial dinner speech, Dewey (“Remarks” 1941: viii) explained that he demurred at the suggestion he become managing editor of *AER* “on the ground that my chief interest was in American economic problems and not in the refinements of economic theory. My acquaintance with theory was limited to some knowledge of Adam Smith, John Stuart Mill, Karl Marx, and Francis A. Walker. I had tried to keep pace with the newer Marshallian analysis and to reconcile the reasoning of the Austrian school with the antiquated concepts of pre- and mid-Victorian economists. And such economics as I had imbibed was imbedded in a thick layer of Vermont G.O.P. Thus you can see that I was but poorly qualified to assume the editorial responsibilities which the year 1910 demanded. My shortcomings in theory were met by the answer that the *Quarterly Journal of Economics* ably took care of theory.” Later, he points out that “[f]or thirty years I have been occupied in reading manuscripts and giving hasty and regrettably only a superficial consideration to the thousands of volumes published in our field of study. These volumes may be roughly divided into two classes: (1) factual studies or reports of investigations; (2)

speculative and theoretical discussions of the working of economic principles or laws. With the first class I have no concern. They are all welcome, for they furnish the data needed for the second class. It may be that some of the authors of this first class are not necessarily economists in the true sense of the term. They are machinists who do the mechanical work for the engineers. My special interest is in the authors of the second class. What is the competency of writers on economic theory; and what sort of training have they had to warrant their prescription for social welfare? Have they some sort of certificate which warrants the appellation “economist”? Now it must be recognized that our universities have attempted to solve this question of certification by laying out courses leading to a doctoral degree, and there is a growing tendency for administrative officers to demand this certification as a price for appointments. This brings us face to face with the character and quality of the Ph.D. degree in the field of economics. Does this degree represent the accumulation of a certain number of credits in detached or scattered economic subjects, supplemented by a so-called research termed a thesis; or does it represent a ripening of scholarship in the wide expanse of economic life? Is not the curriculum for the Ph.D. degree frequently too strictly confined to economic subjects, and should it not include a larger proportion of history, politics, psychology, philosophy, and jurisprudence? Is not a knowledge of American and foreign constitutional history an absolute requisite for understanding and interpreting the working of economic principles? If there were this knowledge, it seems to me that some of our economic theorizing and writing would be more effective” (ix–x.). Coats (1969: 59) records a similar statement by Dewey: “we have been living too much on theoretical hypotheses in the past, and there is a vast amount of testing to be done before we are going to make substantial progress in understanding economic structure.” Nonetheless, Dewey was criticized for making *AER* a “journal unreceptive to the growing technical rigor and formalization of economics,” an effect, some suggest, that “was a

good deal stronger on the *AER* than on the profession. In effect Dewey subsidized the rise of *Econometrica*” (Stigler, Stigler, and Friedland 1995: 344).

According to Stigler, Stigler, and Friedland (1995: 334), economic theory is “the authoritative central core of economics, and even the many applied economists who will not employ the advanced techniques are expected to maintain some familiarity with what is evolving in economic theory... As a consequence of the dominant status of economic theory, the major journals all emphasize this subject as their central agenda and thereby reinforce their status.... Conversely, none of the major journals is able to fill its pages only with articles that have primary or substantial theoretical content, although a few come fairly close. And conversely again, major theoretical contributions appear occasionally in journals outside the central core of the discipline.” Samuelson (2004: 49) also recounts that when he “began the study of economics back in 1932 on the University of Chicago Midway, economics was literary economics. A few original spirits—such as Harold Hotelling, Ragnar Frisch, and R. G. D. Allen—used mathematical symbols; but, if their experiences were like my early ones, learned journals rationed pretty severely acceptance of anything involving the calculus. Such esoteric animals as matrices were never seen in the social science zoos. At most a few chaste determinants were admitted to our Augean stables. Do I seem to be describing Eden, a paradise to which many would like to return in revulsion against the symbolic pus-pimples that disfigure not only the pages of *Econometrica* but also the *Economic Journal* and the *American Economic Review*? Don’t believe it. Like Tobacco Road, the old economics was strewn with rusty monstrosities of logic inherited from the past, its soil generated few stalks of vigorous new science, and the correspondence between the terrain of the real world and the maps of the economics textbook and treatises was neither smooth nor even one-to-one.” In discussing the role of mathematics, Baumol (1991: 2) refers to a time when authors were “expected to begin with a few words of apology, arguing, or at least asserting, that

employment of this tool did not necessarily make the resulting work less ‘realistic’ or less relevant. Even so, it was customary for the algebra to be relegated to an appendix where it would not disturb the sensibilities of the normal reader.” He further recalls working with some determination to change this situation (e.g., being in favor of some grounding in mathematics as a standard of a postgraduate curriculum). Nonetheless, Baumol also levels the criticism that “things may have gone a bit far in the opposite direction,” pointing out that “few specialised students are allowed to proceed without devoting a very considerable portion of their time to the acquisition of mathematical tools, and they often come away feeling that any piece of writing they produce will automatically be rejected as unworthy if it is not liberally sprinkled with an array of algebraic symbols” (2). Thus, researchers may have experienced a shift in the requirements for a publishable paper. Indeed, Frey (2005: 181) reports an interesting personal communication on May 14, 2002, with Robert H. Frank, whose “If Homo Economicus Could Choose His Own Utility Function, Would He Want One with a Conscience?” was published in *AER* in 1987. Originally, this article consisted of two parts: “The first part contained what he really wanted to convey to the readers, and the second part was a formal appendix Frank himself did not find of much importance. One of the referees demanded that the first part be deleted, and that the appendix (which was clearly less interesting according to Frank) essentially constitute the entire paper. Frank agreed, because he knew that this was the price he had to pay to have the paper accepted by the *AER*.”¹⁰⁶ This

¹⁰⁶ The publication in *AER* of a theoretical comment by Joseph E. Harrington (1989: 593–594) that contains three propositions and proofs followed two years later. Harrington concludes with the statement that “the behavior that Frank sought to explain certainly represents an important and interesting class of behaviour with which economists and more generally, social scientists should be concerned. It is a weakness to the neoclassical approach that it has been unable to provide an adequate explanation for such behaviour. Though the approach of Frank offers promise toward providing such an explanation, it appears that it is quite sensitive to relatively small modifications of the behavioural model. While we find that cooperation can indeed emerge as part of an evolutionarily stable outcome, the fact is it need not emerge and when it does emerge it will probably have a difficult time doing so.” Frank (1989: 594) provides the following response: “In my model of the evolution of honesty, I assumed the existence of a signal — a blush, perhaps—extreme values of which served to identify some individuals as being honest with certainty. Joseph Harrington notes that without this assumption, honest individuals have difficulty invading a population initially dominated by defectors. For readers who do not wish to work through the algebra in his comment, the argument is easily summarized in nontechnical terms. Suppose

incident is only one example of the influence exerted by the preference for technical expression.¹⁰⁷ Nonetheless, we suspect it could be interesting to collect data on referees' comments to assess whether such tendencies are actually observable or not. As Baumol (1991: 2) acknowledges, mathematical methods have provided invaluable contributions in many economic fields and there is "no reason to impede or discourage the work of even the most abstraction-minded and esoteric of mathematical economists." However, the "trouble is that if individuals are not respected for the pursuit of alternative approaches, if only those whose writings are pockmarked by algebraic symbols receive kudos, one can expect a misallocation of resources like that which always results from a distortion of relative prices." Moreover, "not only can we expect more than optimal amount of study and publication to be based on mathematical methods, but we can expect people to be induced to adopt this approach even though they are relatively poorly endowed with the requisite talents. Graduate programmes, for example, will be burdened with a spate of dissertations that qualify primarily as mathematical (or econometric) exercises whose sole *raison d'être* seems to be the opportunity they afford to their authors to display whatever facility they can muster in manipulation of the tools of abstraction. Even the most mathematically-oriented of our colleagues will undoubtedly agree that this is what has already happened" (3).

two honest mutants, A and B, arrive in an uncountably large population consisting entirely of dishonest persons. And suppose that the probability that an honest person exhibits an intense blush is, say, 0.999, while the corresponding probability for everyone else is only 0.001. When A sees an intense blush on the face of B, what will then be his estimate of the probability that B is honest? Assuming that A knows the laws of elementary probability and corrects for the base rate of honest persons in the population, it will be zero. When virtually everyone in the population is dishonest, even a person with an intense blush will be pegged as dishonest, provided that even the smallest fraction of dishonest persons also shows an intense blush. Without a fail-safe signal whereby A and B could identify one another as being honest, each would expect that it would not pay to interact with the other. And so it follows that the honest mutants could not reap the fruits of cooperation under these circumstances. If, however, A and B had a sure way of identifying one another as honest persons, they could then interact selectively with one another and make headway against the rest of the population." He concludes by saying that "[i]n any event, there is surely no doubt that many honest persons have managed to make their way into the population somehow. And given that they have, the signaling mechanism in my model will operate just as described, whether or not it is possible to say with certainty that any specific individual is honest."

¹⁰⁷ We would like to note that we are confident that there are many examples out there in which the opposite has occurred.

In 1957, a survey sent to members by the association¹⁰⁸ did indeed indicate the presence of sentiments in favor of less technical papers. Twenty years later, to explore the attitudes and needs of the users of economics journals, Stephen Kagann and Kenneth W. Leesson (1978) also conducted a survey among 15,860 AEA members and managed to gather a total of 455 useable responses. This survey evaluates the technical levels of articles by collecting data on economists' attitudes toward mathematical and statistical exposition in the published literature. First, it asks respondents whether they feel that each journal is "too technical," "about right," or "not rigorous enough." It also includes items that explore the relationship between the number of mathematical expressions and the number of readings of each article appearing in the selected journals list. The results indicate that 50% of the respondents judged *AER* to be "too technical," but just 16% felt that way about *JPE* and only 21% about *QJE*. Only *Econometrica* received more criticism: 61% of the respondents found the journal too technical.¹⁰⁹ This result surprised the researchers: "It had been our impression that the technical level of these journals are [sic] roughly similar. Apparently, many of the respondents, perhaps because they are more familiar with *AER* which they receive on a regular basis, are dissatisfied with the 'too technical' level of the articles published in this particular journal" (989). The survey results also indicate that students and economists connected to research organizations were much less likely than the entire group of respondents to describe a journal as "too technical." On the other hand, business and government economists were far more likely to consider the journals in the survey as too technical.¹¹⁰ Criticism of the technical level of publications in *AER* during the late 1970s led

¹⁰⁸ A random sample of 578 derived by taking every 14th name (U.S. and Canada) from a directory (response rate: 43%), together with a selected sample of members who wrote book reviews for *AER* in 1956 and 1957 (two issues), which covered 103 authors (73 of whom responded) published in the *Papers and Proceedings* of May 1958.

¹⁰⁹ Other journals: *JEL*, *EJ*, *ReStat*, *ReStud*, *JMCB*, *JF*, *JHR*, *IR*, and *MLR*.

¹¹⁰ Sixty-five percent of the respondents were affiliated with academic institutions.

to the creation in 1987 of a new journal, the *Journal of Economic Perspectives* (Diamond 1988).

Although few papers explore in detail the levels of technique used in journals, one exception is the study by Stigler, Stigler, and Friedland (1995: 339, 342), in which they correctly point out that it “is not easy to characterize precisely the level of technique employed in an article: it may employ advanced techniques only at one point and hence be virtually fully comprehensible to a reader untrained in mathematics. Even when an article’s text is wholly verbal, on the other hand, it will usually employ regression techniques or much more advanced econometrics.” These authors’ results, summarized in appendix table A9, show a significant movement toward the use of mathematics over the past few decades. For example, in the 1922–1923 period, only 1% of the total articles in the journals they explored used geometry and only 2% used algebra and/or econometrics. By 1989–1990, however, 37.4% of the total articles published in *AER* used algebra and/or econometrics (*JPE* 26.1%, *QJE* 50%). Likewise, in 1922–1923, only 2% of the total articles in *AER*, *JPE*, and *QJE* used calculus or more advanced techniques, but by 1989–1990, 52.3% of *AER* articles included calculus or more advanced techniques (*JPE* 65.2%, *QJE* 44.4%). Thus, only 10.3% of the total articles analyzed for the 1989–1990 period used primarily verbal techniques.

To extend this analysis, we examine *AER* content not simply in terms of *whether* an article contains algebra and/or econometrics but focusing also on *how often* equations, tables, or figures appear in the papers. To identify changes over time, we also explore two time periods: 1984–1988 and 2004–2000. Use of equations serves as a proxy for the mathematical tools used, and the number of figures and tables proxies for the application of statistical and econometric tools.

TABLE 19: NUMBER OF EQUATIONS, TABLES, AND FIGURES IN 1,082 *AER* ARTICLES

Variables	Period 1984 to 1988 <i>N</i> = 585	Period 2004 to 2008 <i>N</i> = 497	<i>z</i> - score
Equations			
Numbers of equations in the main text per article	11.9	14.64	4.881
Number of equations in the appendix per article	1.37	7.49	10.006
Total equations per article	13.27	22.13	6.85
Total equations/length of article	1.27	1.11	-0.301
Tables			
Tables in the main text per article	1.73	3.30	8.195
Tables in the appendix per article	0.06	0.22	6.405
Total tables per article	1.79	3.52	8.834
Total tables/length of article	0.16	0.18	3.664
Figures			
Figures in main text per article	0.96	3.00	13.235
Figures in appendix per article	0.03	0.07	3.101
Total figures per article	0.98	3.07	16.586
Total figures/length of article	0.1	0.15	9.584

Notes: We consider an equation to be a mathematical formula if it is either numbered in the article or is clearly separated from the text (e.g., placed on a line by itself). We test the statistical significance of the differences between 1984–1988 and 2004–2008 using two-sample Wilcoxon rank-sum (Mann-Whitney) tests.

The results of our analysis, presented in table 19, show that in 1984–1988, a paper contained on average 11.9 equations in the main text and 1.37 equations in the appendix. By 2004–2008, these figures had increased to 14.64 in the main text and 7.49 in the appendix. In both cases, the difference between 1984–1988 and 2004–2008 is statistically significant. One might ask, therefore, whether this difference is due to the length of the articles. However, when we correct for article length, the total number of equations per article (main text and appendix) shows only a very small decrease that is not statistically significant. The number of tables in the main text, on the other hand, increases from 1.73 to 3.30, a difference that is statistically significant. We observe a similar increase in the use of tables in the appendix, one that might again be due to an increase in article length. However, after correcting for this

latter, we still observe a statistically significant increase in the use of tables between the two periods. The same trend is observable for figures: the number of figures per article increases from 0.98 to 3.07 in the main text, and the effect remains robust even when article length is controlled for. This outcome indicates that over time, there has been an increase in the use of applied econometrics or empirical approaches by the authors publishing in *AER*.

In tables 20 and 21, we explore the factors that influence the levels of technique used with simple OLS regressions. Interestingly, we observe that female groups use fewer equations and more tables. We also observe a nonlinear relationship between academic age and total equations used (i.e., an increase at a decreasing rate). On the other hand, a reverse nonlinearity emerges for the number of tables used. Moreover, articles with four or more authors use fewer equations and more tables than single-authored contributions. An author's average ranking position, however, seems barely to matter (the coefficient is only statistically significant for figures). A breakdown of subject area based on technique level identifies "macroeconomics and monetary economics" as the group leader, a position that is mainly driven by the number of equations. When the analytical basis is the number of tables, "health, education, and welfare" tops the subject area group with "microeconomics" at the bottom. Interestingly, "mathematical and quantitative methods" leads the subject-area group for the total number of figures, followed by "macroeconomics and monetary economics." Notably, the time dummy 2004–2008 is statistically significant for the technique level equations and figures.

TABLE 20: DETERMINANTS OF TECHNIQUE IN *AER* ARTICLES (OVERALL AND EQUATIONS)

Independent Variables	Dep. Variable: Total Technical Level (Equations/Figures/Tables)		Dep. Variable: Total Equations	
	[1]	[2]	[3]	[4]
Length	1.723*** <i>18.91</i>	1.76*** <i>20.27</i>	1.332*** <i>13.67</i>	1.382*** <i>14.88</i>
Academic Age	0.205 <i>1.46</i>	0.123 <i>0.96</i>	0.311** <i>2.06</i>	0.208 <i>1.55</i>
Academic Age Squared	-0.009*** <i>-2.73</i>	-0.008** <i>-2.51</i>	-0.011*** <i>-2.97</i>	-0.009*** <i>-2.81</i>
Rank Average	-0.025 <i>-0.94</i>	-0.029 <i>-1.13</i>	-0.038 <i>-1.34</i>	-0.0405 <i>-1.51</i>
Female Share	-11.667*** <i>-5.27</i>	-9.461*** <i>-4.34</i>	-12.796*** <i>-5.31</i>	-10.389*** <i>-4.34</i>
One Coauthor	1.631 <i>1.38</i>	1.028 <i>0.93</i>	1.903 <i>1.53</i>	1.223 <i>1.05</i>
Two Coauthors	-0.046 <i>-0.02</i>	-0.83 <i>-0.45</i>	-0.953 <i>-0.46</i>	-1.716 <i>-0.87</i>
Three or More Coauthors	-10.308*** <i>-3.71</i>	-10.936*** <i>-3.83</i>	-12.229*** <i>-4.49</i>	-13.133*** <i>-4.44</i>
2004 – 2008 Dummy	-3.12** <i>-2.2</i>	-2.407* <i>-1.68</i>	-2.944** <i>-1.99</i>	-2.5* <i>-1.68</i>
(A) General Economics and Teaching		-1.692 <i>-0.62</i>		-1.55 <i>-0.54</i>
(B) Schools of Economic Thought and Methodology		-7.592*** <i>-3.9</i>		-7.521*** <i>-3.75</i>
(C) Mathematical and Quantitative Methods		-7.163*** <i>-4.38</i>		-8.298*** <i>-4.61</i>
(D) Microeconomics		1.863 <i>1.52</i>		3.24** <i>2.55</i>
(E) Macroeconomics and Monetary Economics		6.507*** <i>3.82</i>		6.608*** <i>3.71</i>
(F) International Economics		0.181 <i>0.11</i>		0.338 <i>0.19</i>
(G) Financial Economics		-2.792 <i>-1.38</i>		-2.861 <i>-1.31</i>
(H) Public Economics		-2.489 <i>-1.54</i>		-2.015 <i>-1.17</i>
(I) Health, Education and Welfare		-9.944*** <i>-5.67</i>		-10.591*** <i>-6.12</i>
(J) Labor and Demographic Economics		-3.439*** <i>-2.66</i>		-4.167*** <i>-3.09</i>
(K) Law and Economics		-2.256 <i>-0.72</i>		-0.961 <i>-0.29</i>
(L) Industrial Organization		-3.461*** <i>-2.71</i>		-3.336** <i>-2.45</i>
(M) Business Administration and Business Economics; Marketing; Accounting		-3.474 <i>-1.15</i>		-3.099 <i>-1.02</i>
(N) Economic History		-15.144*** <i>-6.66</i>		-15.634*** <i>-6.57</i>
(O) Economic Development, Technological Change, and Growth		-2.085 <i>-1.05</i>		-2.702 <i>-1.27</i>
(P) Economic Systems		-6.857*** <i>-2.87</i>		-8.228*** <i>-3.23</i>
(Q) Agricultural, Natural Resource: Environmental, Ecological Economics		0.358 <i>0.13</i>		0.1703 <i>0.06</i>
(R) Urban, Rural, and Regional Economics		-0.898 <i>-0.34</i>		-1.06 <i>-0.38</i>
(Z) Other Special Topics		-7.654 <i>-2.2</i>		-5.783 <i>-1.49</i>
Obs	1072	1071	1072	1071
R ²	0.426	0.493	0.294	0.381

Notes: Includes short papers (e.g., notes, comments, replies) but excludes articles in the *Papers and Proceedings*. Coefficients in bold and *t*-statistics in italics. To obtain robust standard errors in these estimations, we use the Huber/White/Sandwich estimators of standard errors. The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. Since our ranking (adopted from Amir and Knauff 2008) excludes many universities in the top 58, we allocate these universities a constant value of 59. The higher the ranking value, the lower the performance. Academic age: year of Ph.D. – year of publication.

TABLE 21: DETERMINANTS OF TECHNIQUE IN *AER* ARTICLES (TABLES AND FIGURES)

Independent Variables	Dep. Variable: Total Tables		Dep. Variable: Total Figures	
	[1]	[2]	[3]	[4]
Length	0.202*** <i>14.61</i>	0.193*** <i>14.63</i>	0.16*** <i>11.82</i>	0.156*** <i>11.39</i>
Academic Age	-0.067*** <i>-3.13</i>	-0.055*** <i>-2.68</i>	-0.033 <i>-1.23</i>	-0.02 <i>-0.78</i>
Academic Age Squared	0.001* <i>1.79</i>	0.001 <i>1.32</i>	0.001 <i>0.92</i>	0.0004 <i>0.56</i>
Rank Average	0.005 <i>1.15</i>	0.005 <i>1.39</i>	0.009** <i>2.55</i>	0.008 <i>2.15</i>
Female Share	1.546*** <i>3.67</i>	1.104*** <i>2.83</i>	-0.171 <i>-0.41</i>	0.021 <i>0.05</i>
One Coauthor	-0.155 <i>-0.87</i>	-0.085 <i>-0.49</i>	-0.191 <i>-1.11</i>	-0.19 <i>-1.11</i>
Two Coauthors	0.526* <i>1.75</i>	0.576** <i>1.97</i>	0.324 <i>1.06</i>	0.255 <i>0.85</i>
Three or More Coauthors	2.168*** <i>4.61</i>	2.331*** <i>4.76</i>	-0.25 <i>-0.37</i>	-0.211 <i>-0.3</i>
2004 – 2008 Dummy	-0.319 <i>-1.53</i>	-0.165 <i>-0.82</i>	0.606*** <i>3.46</i>	0.656*** <i>3.53</i>
(A) General Economics and Teaching		-0.113 <i>-0.27</i>		0.408 <i>0.53</i>
(B) Schools of Economic Thought and Methodology		0.458 <i>0.68</i>		-0.315 <i>-0.78</i>
(C) Mathematical and Quantitative Methods		0.466 <i>1.55</i>		0.977*** <i>2.99</i>
(D) Microeconomics		-1.086*** <i>-6.12</i>		-0.156 <i>-0.88</i>
(E) Macroeconomics and Monetary Economics		-0.587*** <i>-2.6</i>		0.447** <i>1.98</i>
(F) International Economics		0.0005 <i>0.00</i>		-0.195 <i>-0.86</i>
(G) Financial Economics		0.796** <i>2.26</i>		-0.4 <i>-1.39</i>
(H) Public Economics		0.01 <i>0.04</i>		-0.56*** <i>-3.09</i>
(I) Health, Education and Welfare		1.608*** <i>4.35</i>		-0.707** <i>-2.39</i>
(J) Labor and Demographic Economics		0.599*** <i>2.81</i>		0.052 <i>0.24</i>
(K) Law and Economics		-0.596 <i>-0.93</i>		-0.645* <i>-1.69</i>
(L) Industrial Organization		0.233 <i>1.08</i>		-0.193 <i>-1.1</i>
(M) Business Administration and Business Economics; Marketing; Accounting		-0.198 <i>-0.46</i>		-0.391 <i>-1.29</i>
(N) Economic History		0.324 <i>0.71</i>		0.584 <i>1.03</i>
(O) Economic Development, Technological Change, and Growth		0.421 <i>1.33</i>		0.208 <i>0.79</i>
(P) Economic Systems		1.469** <i>2.23</i>		0.174 <i>0.51</i>
(Q) Agricultural, Natural Resource: Environmental, Ecological Economics		0.086 <i>0.21</i>		0.241 <i>0.65</i>
(R) Urban, Rural, and Regional Economics		-0.107 <i>-0.24</i>		0.501 <i>0.94</i>
(Z) Other Special Topics		-0.225 <i>-0.33</i>		-1.429*** <i>-3.23</i>
Obs	1072	1071	1072	1071
R ²	0.313	0.387	0.29	0.325

Notes: Includes short papers (e.g., notes, comments, replies) but excludes articles in the *Papers and Proceedings*. Coefficients in bold; and *t*-statistics in italics. To obtain robust standard errors in these estimations, we use the Huber/White/Sandwich estimators of standard errors. The symbols *, **, *** represent statistical significance at the 10%, 5% and 1% levels, respectively. Since our ranking (adopted from Amir and Knauff 2008) excludes many universities in the top 58, we allocate these universities a constant value of 59. The higher the ranking value, the lower the performance. Academic age: year of publication – year Ph.D. obtained.

We finalize our investigation by reporting the level of technique used by the most cited *AER* articles (see table 7). Our results indicate that 5 out of the 11 articles include equations in the main text (mean = 14.9 equations per article, 1.07 equations per page), although only 2 employ equations in the appendix. Only one article includes tables in the main text (11 of them), but none places tables in the appendix. On the other hand, 5 articles include figures in their main text (mean = 2.82 figures per article, 0.18 per page). The maximum number of figures used in the main text is 12, and only one article includes figures in the appendix. Finally, although the mean values for equations and figures are comparable to the averages for 2004–2008 articles, there substantial heterogeneity among these top 11 papers.

8. *Conclusions*

The fact that the *American Economic Review* is celebrating its centennial encouraged us to take a closer look at how the journal has developed over the past 100 years. The topics chosen were driven primarily by *personal* interest rather than a desire for completeness or for the purpose of developing a coherent argument.

To characterize the successful 2011 *AER* author, we apply forensic profiling to the evidence reported above: This author is male, with a Ph.D. from MIT (obtained in 2007), a B.A. from the University of Chicago, and an M.A. from Harvard University. He likes to socialize with former MIT students who are still in academia. Most of these are older and active as editors or coeditors and are interested in keeping up to date with his research topics, which in the past enabled them to get successful papers from him. He works in Israel, visits the U.S. regularly, and has a passport from Switzerland. Because he is happy with this arrangement, he does not really follow job openings. He is married, and his wife has a professorship in humanities, although she has just experienced career interruption due to childbearing. She also stresses that her husband has a female side in him that is now

sustainably growing. He is currently cooperating intensively with Armen A. Alchian and Harold Demsetz on a paper that he believes could be quite successful, and is also working with Joseph Stiglitz on three other papers. He has been productive since the beginning of his career and has already won the John Bates Clark Medal. He has published extensively with both William J. Baumol and Joseph E. Stiglitz, preferring to team up with two superstars rather than only one. He is also involved in projects with researchers such as Kenneth J. Arrow, Angus S. Deaton, John Muellbauer, Peter A. Diamond (several), James A. Mirrlees, Avinash K. Dixit, Sanford Grossman, Dale Jorgenson, Anne O. Krueger, Paul Krugman, Robert E. Lucas, Jr., Robert A. Mundell, Stephen A. Ross, and Robert J. Shiller. Many researchers expect him to win the Nobel Prize. He is increasingly willing to publish in groups. Instead of going to the Annual Meeting of the American Economic Association, he prefers to discuss papers in detail with Gary S. Becker (also following his advice on the contribution's title). He has read the entire work of Frank H. Knight and has even procured lecture notes from former Knightian students who wrote their professor's words down in detail in order to fully understand the profundity of his lectures. He regularly discusses the potential of Knight's work and ideas with James Buchanan. His last name is positioned in the alphabet such that when he publishes with two or more coauthors, the papers often have a nonalphabetical name ordering in recognition of his ability to oversee the entire paper. However, in recent years, he has been more willing to accept alphabetical name ordering. He publishes only main articles and likes to work with figures (increasingly so in recent years). He has recently been promoted from assistant professor to professor, and his colleagues describe him as someone who seeks prestige. He is, however, concerned about inequality because his success has led to an unequal distribution of publications and citations. He does not mind that his research may produce some dry holes. In fact, some of his publications have not so far been cited at all. Two years ago, he received the *AER* Excellence in Refereeing

Award, and according to the IDEAS ranking, he is among the top 5% researchers. On average, his contributions are around 10 pages, although the length can vary. Most particularly, his papers can become substantially longer when he works in such subject areas as financial economics and labor and demographic economics. In the past, his papers were shorter, but his papers tend to be longer when he works in groups of three. He is also growing substantially more interested in such subject areas as economic history; health, education, and welfare; microeconomics; and mathematical and quantitative methods.. On the other hand, he has lost interest in macroeconomics and monetary economics, an area in which, his former friend Robert A. Moffitt points out, he was never really a leader. When he collaborates with women, he uses fewer equations and more tables. On the other hand, when he works in the area of microeconomics and monetary economics, he uses a substantial number of equations. He also uses tables more frequently when working in health, education, and welfare. When he collaborates with superstars, he only uses equations in the main text about half the time and in most cases, barely at all.

Clearly, the analysis reported in this paper has several shortcomings, not least of which is our exclusive focus on the history of *AER*. Thus, the results are not representative of the entire economics discipline. In addition, the paper is overloaded with tables and figures, which may produce an end product similar to that described by Farquhar and Farquhar (1891: 55, as cited in Stigler, Stigler, and Friedland 1995: 344), a paper that is “grievously wearisome to the eye” and from which “the popular mind is as incapable of drawing any useful lessons ... as of extracting sunbeams from cucumbers.” We hope, however, that by focusing on publication data for *AER*, we have revealed a valuable structure. Another shortcoming is that the material published in *AER* is endogenous; however, as the *overall* sketch of this contribution indicates, we claim no causal relationships. Rather, in general, we are only trying to glimpse the “peak of the iceberg,” which we do using a primarily

descriptive analysis. To dig deeper would require the type of cooperation from journals as was given to Rebecca M. Blank (1991), who used several years of *AER* data as the basis for her interesting randomized experiment.¹¹¹ Her study provides an excellent example of the possibilities once a researcher secures such cooperation. For our study, in contrast, most material was manually and single-handedly collected from published *AER* articles and from statistics available in the annual Report of the Editor. The Web also served as a valuable sources for author, editor, coeditor, board member, and referee profiles. As a Delphic Oracle,¹¹² we relied on *Journal Citation Reports*.

The most cited *AER* contribution by Alchiam and Demsetz (1972: 779) (see table 7) begins its Team Production section as follows: “Two men jointly lift heavy cargo into trucks. Solely by observing the total weight loaded per day, it is impossible to determine each person’s marginal productivity. With team production it is difficult, solely by observing total output, to either define or determine *each* individual’s contribution to this output of the cooperation input. The output is yielded by a team, by definition, and it is not a *sum* of separable outputs of each of its members. Team production of Z involves at least two inputs, X_i and X_j , with $\partial^2 Z / \partial X_i \partial X_j \neq 0$. The production function is *not* separable into two functions each involving only inputs X_i or only input X_j . Consequently there is no sum of Z of two separable functions to treat as the Z of the team production function ... There exist production techniques in which the Z obtained is greater than if X_i and X_j had produced

¹¹¹ One-half of the submitted papers were assigned to single-blind reviewing, while the other half were assigned to double-blind reviewing. The difficulties of such an experimental design is to keep it secret within the economics profession: “Thus, referees who knew that such an experiment was ongoing, might have behaved somewhat differently when they reviewed and evaluated papers that they would in the absence of such an experiment” (Blank 1991: 110).

¹¹² *Wikipedia*, in an entry on the Oracle of Delphi, makes the following comment on its history: “There are also many later stories of the origins of the Delphic Oracle. One late explanation, which is first related by the 1st century BC writer, Diodorus Siculus, tells of a goat herder named Coretas, who noticed one day that one of his goats, who fell into a crack in the earth, was behaving strangely. On entering the chasm, he found himself filled with a divine presence and could see outside of the present into the past and the future. Excited by his discovery he shared it with nearby villagers. Many started visiting the site to experience the convulsions and inspirational trances, though some were said to disappear into the cleft due to their frenzied state” (see <http://en.wikipedia.org/wiki/Pythia>).

separable Z. Team production will be used if it yields an output enough larger than the sum of separable production of Z to cover the costs of organizing and disciplining team members—the topics of this paper. Usual explanations of the gain from cooperative behavior rely on exchange and production in accord with the comparative advantage specialization principle with separable additive production. However, as suggested above there is a source of gain from cooperative activity involving working as a *team*, wherein individual cooperating inputs do not yield identifiable, separate products which can be *summed* to measure the total output Team production, to repeat, is production in which 1) several types of resources are used and 2) the product is not a sum of separable outputs of each cooperating resource. An additional factor creates a team organization problem—3) not all resources used in team production belong to one person.”

If indeed team production is not the sum of separable outputs from each of its members, then a clear name ordering of authors, be it alphabetical or otherwise, may be questionable. As discussed in this paper, even an alphabetical ordering has real consequences. Why, then, do we not delete all the individual names when referencing the work of others? For example, one could apply the referencing style of scientific journals such as *Science* or *Nature*, which requires that references each be numbered in the main text in the sequence in which sequentially as they appear. Given that papers can be found using just the title, volume, and page numbers, one could even go further by deleting all the authors’ names in the references: “An extreme and idealistic solution would be to publish scientific contributions anonymously. After all, it should not matter *who* wrote an article but only whether its content *enlarges our knowledge*. There are, of course, many problems with such a solution. An obvious one is that scholars would no longer have an extrinsic incentive to publish” (Frey forthcoming: 10). This practice might nonetheless reduce the “cult of personality.” In fact, in his contribution to the *Lives of the Laureates*, Robert M. Solow (2004: 153) adopts just such

an attitude: “To be honest, I should warn you that I am going to tell you as little about myself as I can get away with in a lecture about ‘My Evolution as an Economist.’ My reason is not that I have anything to hide. I wish I had more to hide; that would at least suggest an exciting life. My problem is that I think the ‘cult of personality’ is slowly swamping our culture. You can see it at its most dangerous in presidential elections, where eyebrows seem to be more important than ideas. I tend to blame that on television, which is a better medium for eyebrows than for economic theory. But that sort of technological determinism won’t quite do: it leaves us with the task of explaining the psychologization of almost everything, the success of pop books on character, the fact that seven out of ten nonfiction best sellers are biographies, the importance attached to the ‘personal relationship’ between Mr. Reagan and Mrs. Thatcher. Something pretty deep is going on there. (I don’t mean between Mr. Reagan and Mrs. Thatcher!)... Anyway, what I have called the cult of personality has to be a sign of cultural decay.” Our society, then, has a flair for a cult of personality, and academia may be no exception: “If researchers strive for it, why not let them experience the marginal benefits and costs of this as long as there is a net benefit” (Samuelson 2004: 60). Why not choose, therefore, a less idealistic and efficient strategy that *might* make authors happier. Instead of total anonymity, one could allow authors to add their own signature in the same way that *painters* mark their work.¹¹³ Yet how might this be done? Although we provide one possible answer in figure 18, we found valuable and more comprehensive guidelines on the Web:¹¹⁴

¹¹³ The famous theoretical physicist and mathematician Freeman Dyson (1996: 802-805) has argued: “Science in its everyday practice is much closer to art than to philosophy. When I look at Gödel’s proof of his undecidability theorem, I do not see a philosophical argument. The proof is a soaring piece of architecture, as unique and as lovely as Chartres cathedral. Gödel took Hilbert’s formalized axioms of mathematics as his building-blocks and built out of them a lofty structure of ideas into which he could finally insert his undecidable arithmetical statement as the keystone of the arch. The proof is a great work of art... Gödel proved that in mathematics the whole is always greater than the sum of the parts... The black-hole solution of Einstein’s equation is also a work of art. The black hole is not as majestic as Gödel’s proof, but it has the essential features of a work of art: uniqueness, beauty, and unexpectedness... My message is that science is a human activity, and the best way to understand it is to understand the individual human beings who practise it. Science is an art form and not a philosophical method... Science flourishes best when it uses freely all the tools at hand, unconstrained by preconceived notions of what science ought to be.”

¹¹⁴ <http://painting.about.com/cs/careerdevelopment/a/signpainting.htm>

Adding your signature to a painting is like adding a stamp to it that reads "finished." It's a sign that you're satisfied with the painting and no longer consider it a work in progress.

Is It Really Necessary to Sign a Painting?

It's not a legal requirement, but if you don't add your name to a painting, how will anyone know who the artist is? You may argue that you've a very familiar style that people will recognize, but what if it's the first time someone's encountered your work? How will they find out who the artist is then? If it's hanging in a gallery it'll have a label with your name on it, but what if it's in the lounge of someone who's bought a painting and they can't remember who the artist was? Think about the works by famous artists which are 'rediscovered' every now and then; is this a fate you want to risk for your paintings?

What Should My Signature Look Like?

The most important thing is that people must be able to read it. An illegible signature isn't a sign that you're extremely creative and it doesn't add a level of intrigue to the painting. You're the artist, so let it be known. But at the same time, don't make it look like you're using a stamp. You don't have to sign your whole name on the front of the painting, you could just put your initials but it's wise to put your full name on the back of the painting. The same applies if you use a symbol or monograph; people have to have some way of knowing what it stands for.

Should I Put a Date With My Signature?

I believe you should date a painting, though it needn't be next to your signature on the front. The reason: when you first start painting you'll probably be able to keep track of when you painted a particular painting, but wait until you've several years' worth of paintings, then you'll simply be unable to remember and will have to guess. Serious collectors and galleries like be able to see how a painter's work has developed over the years, so get into the habit of dating your work now. You don't have to put the date on the front of your painting, but could write it on the back (though once it's framed you may not be able to see it). Or put only the year on the front and the month and year you completed it on the back.

I don't buy the argument that putting a date on a painting limits your potential to sell it. Art isn't like food, a product with a sell-buy date. If buyers only wanted the newest and latest work, then how come there's an auction market for contemporary paintings? And if anyone asks why a painting from a few years back hasn't sold, tell them you'd kept it in your personal collection until now because you regard it as a key work.

Where Do I Put My Signature?

It's up to you, though traditionally a signature is put towards one of the bottom corners. A signature should be an integral part of a painting and not detract from the painting. Be consistent about where you put your signature as then when someone next encounters a painting they think by your, they know exactly where to look to check.

What Should I Use to Sign a Painting?

Use whatever you've created the painting in, whether it's pastel, watercolor, whatever. Try to remember to sign the work before you clean your brushes and palette for the last time from a particular painting so you've got a suitable color to hand that will blend in with the work. (I do it with a thin rigger brush.) Having your signature 'match' the painting, rather than it looking like a later addition, also makes it less likely that someone will question the authenticity of the work at some future date (most likely after you're dead and your paintings have increased in value enormously). Avoid adding your signature on top of a layer of varnish as it'll look like you forgot to do it in time (and if you must, keep it small and rather put your full signature on the back).

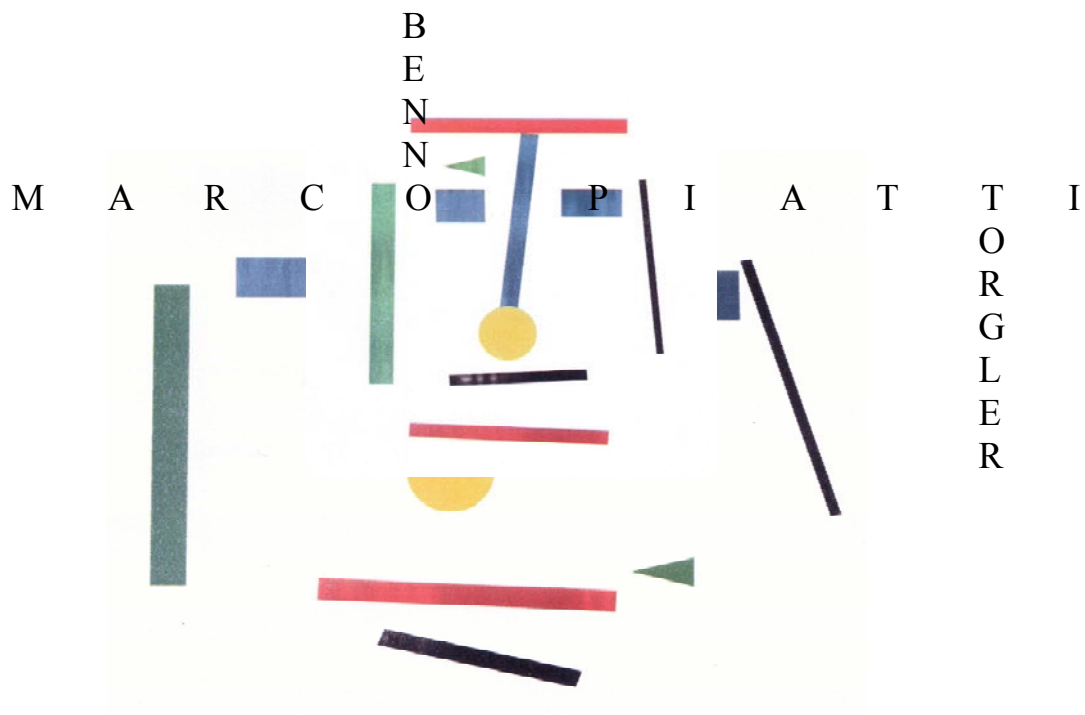
Should You Sign a Painting with Your Maiden Name or Married Name?

If you change your name when you get married, how should you sign your paintings? Should you continue using the name you had been, your maiden name, or should you change to your new, married name? Ultimately, it's a matter of individual preference.

If an artist is already known professionally by a maiden name, it wouldn't make sense to change it because you'd have to remarket yourself. Or if both partners are artists, then sometimes people prefer to have different names to avoid comparison. Using a maiden name certainly solves any problem if a divorce later happens, but it's hard to say to a new partner because it implies a lack of belief in a relationship, which it isn't the issue it's tied into at all. Your personal identity as an artist may be strongly tied into the name you've had since birth.

There is no correct way or choice when it comes to signing a painting with your maiden name or not, it's an individual choice.

FIGURE 18: EXAMPLE OF HOW COOPERATION AMONG AUTHORS MIGHT BE VISUALIZED IN A WORLD WITH NO CLEAR AUTHOR ORDERING.



Notes: The design tool is available on Jonathan Feinstein's personal homepage (see <http://www.jonathanfeinstein.com/>).

“(...) working out economic analysis is play, not work” (Samuelson 2004: 59).

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APPENDIX

TABLE A1: A SUMMARY OF INSTITUTIONAL RANKINGS

University	Appearance as a Top 10 University	Appearance as a Top 20 University
Massachusetts Institute of Technology	29	29
Harvard University	27	28
University of Chicago	26	27
Stanford University	25	29
Princeton University	23	28
University of California, Berkeley	21	26
University of Pennsylvania	20	25
Yale University	20	23
Northwestern University	15	24
Columbia University	13	22
University of California, Los Angeles	9	24
University of Michigan	9	18
University of Wisconsin	9	22
New York University	5	20
Hebrew University	5	8
Carnegie Mellon University	4	10
University of Washington	4	8
Brown University	3	9
University of Western Ontario	3	10
Cornell University	3	13
London School of Economics	3	13
University of Minnesota	0	12
Rochester University	0	15

Notes: Data from Tom Coupé (2003), table 2 and table 4 (covering two time periods, 1978–1982 and 1996–2001; four different rankings); two tables from Philip E. Graves, James R. Marchand, and Randal Thompson (1982), table 1 and table 2; table 3 from Pantelis Kalaitzidakis, Theofanis P. Mamuneas, and Thanasis Stengos (2003); five from Erkin Bairam (1994), table 1 (*AER* 1985–90), table 2 (*Econometrica* 1985–90), table 3 (*Economic Journal* 1985–90), table 4 (*JPE* 1985–90) and table 5 (*QJE* 1985–90); table 1 from Amir and Knauff (2008); three tables from Stephen Wu (2007), table 2 (*AER*), table 3 (*JPE*), and table 4 (*QJE*) between for the 2000–2003 period; and 12 by John J. Siegfried (1994), table 1 (*AER*, by decade between 1950 and 1989), table 2 (*JPE*, by decade between 1950 and 1989) and table 3 (*QJE*, by decade between 1950 and 1989) and table 2 by Jean Louis Heck (1993).

TABLE A2: EDITORIAL BOARD MEMBERS IN 2010

Name	Female	Issue1	Issue3	Issue4	Issue5
Mark Gertler	0	1	1	1	1
Pinelopi Koujianou Goldberg	1	1	1	1	1
Dirk Krueger	0	1	1	1	1
Alessandro Lizzeri	0	1	1	1	1
Larry Samuelson	0	0	0	1	1
Joel Sobel	0	1	1	1	0
Pol Antràs	0	1	1	1	1
Ralph Braid	0	1	1	1	1
Markus Brunnermeier	0	1	1	1	1
Gary Charness	0	1	1	1	1
Stephen Coate	0	1	0	0	0
Dora Costa	1	1	1	1	1
Miguel Costa-Gomes	0	1	1	1	1
Rachel Croson	1	1	1	1	1
Matthias Doepke	0	1	1	1	1
Janice C. Eberly	1	1	0	0	0
Hanming Fang	0	1	1	1	1
Emmanuel Farhi	0	0	0	0	1
Jesús Fernández-Villaverde	0	1	1	1	1
Simon Gilchrist	0	1	1	1	1
Gita Gopinath	1	1	1	1	1
David Green	0	1	1	1	1
Christian Hellwig	0	1	0	0	0
Igal Hendel	0	1	1	1	1
Ming Huang	0	1	1	1	1
Botond Kozzegi	0	1	1	1	1
Jonathan Levin	0	1	1	1	1
John A. List	0	1	0	0	0
Guido Lorenzoni	0	1	1	1	1
Sydney Ludvigson	1	1	1	1	1
Shelly Lundberg	1	1	1	1	1
Giuseppe Moscarini	0	0	1	1	1
Guido Lorenzoni	0	0	0	1	1
Kaivan Munshi	0	1	1	1	1
Rohini Pande	1	1	1	1	1
Nina Pavcnik	1	1	1	1	1
Nicola Persico	0	1	1	1	1
Ricardo Reis	0	1	1	1	1
Esteban Rossi-Hansberg	0	1	1	1	1
Frank Schorfheide	0	1	1	1	1
Holger Sieg	0	1	1	1	1
Andrzej Skrzypacz	0	1	1	1	1
Balázs Szentes	0	1	1	1	1
Christopher Timmins	0	1	1	1	1
Petra Todd	1	1	1	1	1
Christopher Udry	0	1	0	0	0
Romain Wacziarg	0	1	1	1	1
Leeat Yariv	1	1	1	1	1
Stephen Yeaple	0	1	1	1	1

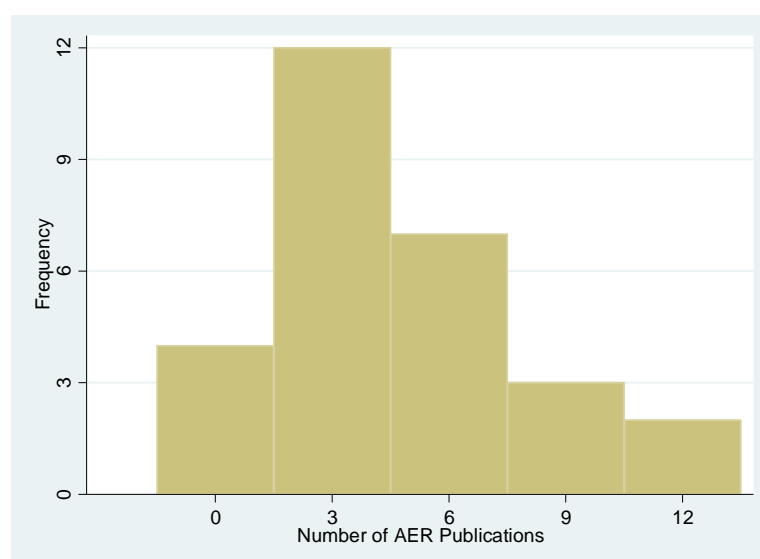
TABLE A3: PROFILE OF COEDITORS

Coeditor Name	Term	University Affiliation at the Time of Starting the Appointment	Ph.D. Affiliation
Larry Samuelson	2010–	Yale University	University of Illinois, Urbana
Dirk Krueger	2009–	University of Pennsylvania	University of Minnesota
Joel Sobel	2009–2010	University of California, San Diego	University of California, Berkeley
Alessandro Lizzeri	2008–	New York University	Northwestern University
Pinelopi Koujianou Goldberg	2007–2010	Yale University	Stanford University
Jeremy I. Bulow	2005–2008	Stanford University	MIT
Mark Gertler	2005–2010	Federal Reserve Bank of New York	Stanford University
Vincent P. Crawford	2005–2009	University of California, San Diego	MIT
Judith Chevalier	2004–2007	Yale University	MIT
B. Douglas Bernheim	2002–2005	Stanford University	MIT
David Card	2002–2004	University of California, Berkeley	Princeton University
Richard Rogerson	2003–2008	Arizona State University	University of Minnesota
Timothy J. Besley	1999–2004	London School of Economics	Oxford University
Valerie A. Ramey	1999–2002	University of California, San Diego	Stanford University
Matthew D. Shapiro	1997–1999	University of Michigan	MIT
Dennis N. Epple	1994–1999	Carnegie Mellon University	Princeton University
Kenneth D. West	1993–1996	University of Wisconsin	MIT
R. Preston McAfee	1993–2002	University of Texas at Austin	Purdue University
John Y. Campbell	1991–1993	Princeton University	Yale University
Roger H. Gordon	1991–1994	University of Michigan	MIT
Paul R. Milgrom	1990–1993	Stanford University	Stanford University
Bennett T. McCallum	1988–1991	Carnegie Mellon University	Rice University
Hal R. Varian	1987–1989	University of Michigan	University of California, Berkeley
John B. Taylor	1985–1988	Stanford University	Stanford University
Robert H. Haveman	1985–1991	University of Wisconsin	Vanderbilt University
John G. Riley*	1983–1987	University of California, Los Angeles	MIT

Notes: We exclude editors who were coeditors at any point in time (e.g., Ben S. Bernanke, Robert A. Moffitt, and Orley Ashenfelter); *from 1983–1984, the title was associate editor. Source:

http://www.aeaweb.org.ezp01.library.qut.edu.au/aer/AER_Editors_Coeditors.pdf.

FIGURE A1: NUMBER OF *AER* PUBLICATIONS BEFORE BECOMING *AER* COEDITOR



Notes: *Paper and Proceedings* articles included.

FIGURE A2: NUMBER OF *QJE*, *JPE*, AND *ECONOMETRICA* PUBLICATIONS BEFORE BECOMING *AER* COEDITOR

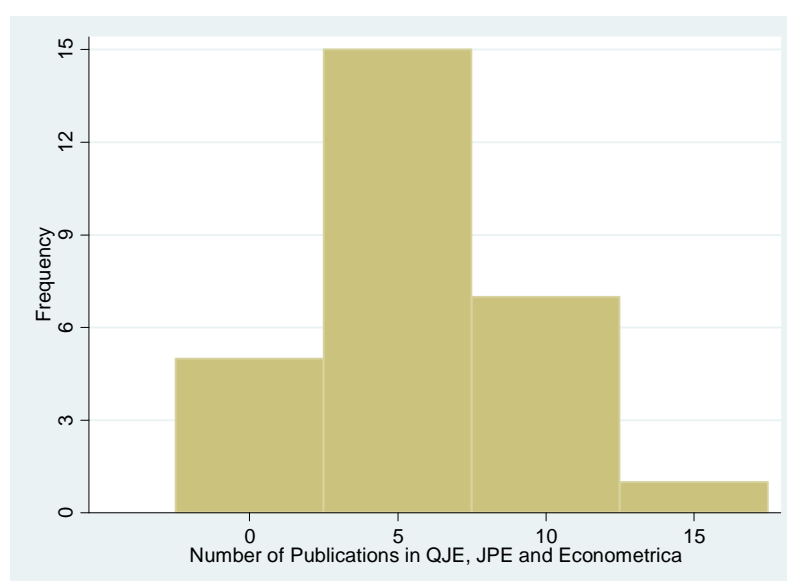
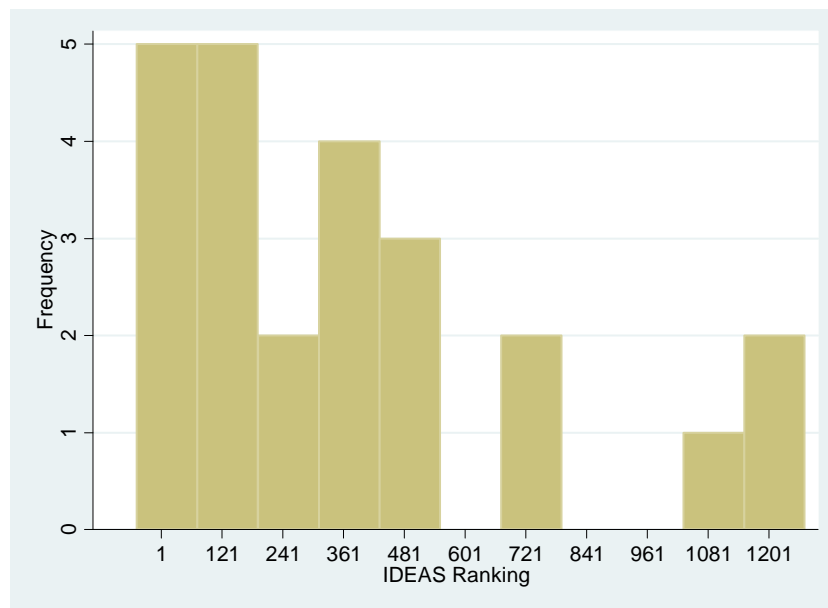


FIGURE A3: COEDITOR'S IDEAS RANKING POSITION IN 2010



Notes: November 2010 ranking, accessed December 2010. Two coeditors are not registered in RePEc's authors list. We exclude editors who were coeditors at any point in time.

TABLE A4A: ROBUSTNESS TEST ON THE RELATIONSHIP BETWEEN CONNECTIONS AND CITATIONS I

Independent Variables:	Dependent Variable: Total Citations		
	[8]	[9]	[10]
University Connection Lag (Current Affiliations)	-1.294 <i>-0.07</i>		
University Life Connection Lag (Past Affiliations)		2.847 <i>0.21</i>	
PhD University Connection Lag			16.51 <i>1.58</i>
Length	4.211*** <i>2.67</i>	4.168*** <i>2.61</i>	4.011** <i>2.48</i>
Main Article (Dummy)	19.112** <i>2.54</i>	18.845** <i>2.38</i>	18.778** <i>2.55</i>
First Article (Dummy)	-29.449* <i>-1.83</i>	-29.693* <i>-1.84</i>	-28.942* <i>-1.79</i>
Female Share	-0.361 <i>-0.04</i>	-0.191 <i>-0.02</i>	1.698 <i>0.19</i>
Academic Age	-0.65 <i>-1.25</i>	-0.62 <i>-1.17</i>	-0.496 <i>-1.03</i>
Authors Reputation	5.987* <i>1.71</i>	5.809 <i>1.61</i>	5.524 <i>1.64</i>
Equations Main Text	-0.41 <i>-1.33</i>	-0.4 <i>-1.29</i>	-0.397 <i>-1.28</i>
Tables Main Text	-0.188 <i>-0.08</i>	-0.171 <i>-0.07</i>	-0.137 <i>-0.06</i>
Figures Main Text	7.024* <i>1.81</i>	7.006* <i>1.79</i>	6.576* <i>1.74</i>
References per Article	0.736 <i>1.26</i>	0.747 <i>1.28</i>	0.788 <i>1.35</i>
Year 1984	13.605 <i>1.13</i>	14.212 <i>1.17</i>	15.843 <i>1.25</i>
Year 1985	18.065 <i>1.59</i>	18.297 <i>1.49</i>	18.959 <i>1.60</i>
Year 1986	25.197** <i>2.36</i>	25.445** <i>2.35</i>	25.639** <i>2.39</i>
Year 1987	21.496** <i>2.11</i>	21.44** <i>2.1</i>	21.19** <i>2.06</i>
One Co-Author	9.926 <i>1.42</i>	10.068 <i>1.43</i>	9.971 <i>1.43</i>
Two Co-Authors	0.295 <i>0.02</i>	0.376 <i>0.02</i>	2.083 <i>0.13</i>
Three or more Co-Authors	57.127 <i>1.10</i>	56.831 <i>1.09</i>	55.65 <i>1.08</i>
JEL Code Variable Included	No	No	No
N	576	576	576
R-squared	0.235	0.235	0.239

Notes: Coefficients in bold, *t*-statistics in italics. To obtain robust standard errors in these estimations, we use the Huber/White/Sandwich estimators of standard errors. The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. Citations accumulated up until September 2008.

TABLE A4B: ROBUSTNESS TEST ON THE RELATIONSHIP BETWEEN CONNECTIONS AND CITATIONS II

Dependent Variable: Total Citations				
Independent Variables:	[11]	[12]	[13]	[14]
University Connection Lag (Current Affiliations)	-3.436 <i>-0.20</i>		-3.31 <i>-0.19</i>	
University Life Connection Lag (Past Affiliations)		0.161 <i>0.01</i>		0.863 <i>0.06</i>
PhD University Connection Lag	16.74 <i>1.59</i>	16.489 <i>1.55</i>	18.494* <i>1.80</i>	18.145* <i>1.75</i>
Length	4.048*** <i>2.60</i>	4.01** <i>2.53</i>	3.966*** <i>2.66</i>	3.914*** <i>2.57</i>
Main Article (Dummy)	18.843** <i>2.52</i>	18.765** <i>2.38</i>	21.09** <i>2.50</i>	20.974** <i>2.41</i>
First Article (Dummy)	-28.905* <i>-1.79</i>	-28.955* <i>-1.79</i>	-20.813 <i>-1.39</i>	-20.85 <i>-1.39</i>
Female Share	1.654 <i>0.18</i>	1.704 <i>0.19</i>	3.595 <i>0.39</i>	3.668 <i>0.40</i>
Academic Age	-0.511 <i>-1.05</i>	-0.495 <i>-1.00</i>	-0.678 <i>-1.45</i>	-0.662 <i>-1.39</i>
Authors Reputation	5.622 <i>1.61</i>	5.516 <i>1.53</i>	5.307 <i>1.57</i>	5.189 <i>1.49</i>
Equations Main Text	-0.404 <i>-1.32</i>	-0.397 <i>-1.29</i>	-0.289 <i>-1.01</i>	-0.28 <i>-0.97</i>
Tables Main Text	-0.164 <i>-0.07</i>	-0.137 <i>-0.06</i>	0.41 <i>0.18</i>	0.442 <i>0.19</i>
Figures Main Text	6.594* <i>1.76</i>	6.576* <i>1.74</i>	6.239* <i>1.65</i>	6.236 <i>1.63</i>
References per Article	0.781 <i>1.36</i>	0.788 <i>1.37</i>	0.834 <i>1.49</i>	0.846 <i>1.50</i>
Year 1984	15.719 <i>1.24</i>	15.871 <i>1.25</i>	19.454 <i>1.53</i>	19.701 <i>1.54</i>
Year 1985	19.111* <i>1.65</i>	18.973 <i>1.53</i>	24.928** <i>2.15</i>	24.795** <i>1.96</i>
Year 1986	25.681** <i>2.39</i>	25.653** <i>2.36</i>	31.785*** <i>2.98</i>	31.772*** <i>2.91</i>
Year 1987	21.338** <i>2.08</i>	21.19** <i>2.05</i>	25.158** <i>2.45</i>	24.954** <i>2.41</i>
One Co-Author	9.933 <i>1.42</i>	9.978 <i>1.42</i>	11.553 <i>1.62</i>	11.63* <i>1.62</i>
Two Co-Authors	2.04 <i>0.13</i>	2.084 <i>0.13</i>	-1.269 <i>-0.08</i>	-1.273 <i>-0.08</i>
Three or more Co-Authors	56.047 <i>1.09</i>	55.644 <i>1.07</i>	57.497 <i>1.14</i>	57.018 <i>1.13</i>
JEL Code Variable Included	No	No	Yes	Yes
N	576	576	576	576
R-squared	0.239	0.239	0.282	0.282

Notes: Coefficients in bold, *t*-statistics in italics. To obtain robust standard errors in these estimations, we use the Huber/White/Sandwich estimators of standard errors. The symbols *, **, *** represent statistical significance at the 10%, 5%, and 1% levels, respectively. Citations accumulated up until September 2008.

TABLE A5: AER EDITORIAL BOARD MEMBERS IN JANUARY 2011

Name	First Year as AER Board Member	University Affiliation	PhD Obtained
Frank Schorfheide	2005	University of Pennsylvania	Yale University
Jonathan Levin	2005	Stanford University	MIT
Gary Charness	2006	University of California, Santa Barbara	University of California, Berkeley
Ming Huang	2006	Cornell University	Stanford University
Rachel Croson	2006	University of Pennsylvania	Harvard University
Ralph Braid	2006	Wayne State University	MIT
Andrzej Skrzypacz	2007	Stanford University	University of Rochester
Holger Sieg	2007	Carnegie Mellon University	Northwestern University
Kaivan Munshi	2007	Brown University	MIT
Shelly Lundberg	2007	University of Washington	Northwestern University
Botond Koszegi	2008	University of California, Berkeley	MIT
Christopher Timmins	2008	Duke University	Stanford University
David Green	2008	University of British Columbia	Stanford University
Dora Costa	2008	University of California, Los Angeles	University of Chicago
Hanming Fang	2008	Duke University	University of California, Berkeley
Miguel Costa-Gomes	2008	University of Aberdeen	University of California, San Diego
Nina Pavcnik	2008	Dartmouth College	Princeton University
Petra Todd	2008	University of Pennsylvania	University of Chicago
Rohini Pande	2008	Harvard University	London School of Economics
Sydney Ludvigson	2008	New York University	Princeton University
Balazs Szentes	2009	University of Chicago	Boston University
Esteban Rossi- Hansberg	2009	Princeton University	University of Chicago
Gita Gopinath	2009	Harvard University	Princeton University
Guido Lorenzoni	2009	MIT	MIT
Igal Hendel	2009	Northwestern University	Harvard University
Jesus Fernandez- Villaverde	2009	University of Pennsylvania	University of Minnesota
Leeat Yariv	2009	California Institute of Technology	Harvard University
Markus Brunnermeier	2009	Princeton University	London School of Economics
Matthias Doepke	2009	Northwestern University	University of Chicago
Nicola Persico	2009	New York University	Northwestern University
Ricardo Reis	2009	Columbia University	Harvard University
Emmanuel Farhi	2010	Harvard University	MIT
Giuseppe Moscarini	2010	Yale University	MIT
Pol Antràs	2010	Harvard University	MIT
Romain Wacziarg	2010	University of California, Los Angeles	Harvard University
Stephen Yeaple	2010	Pennsylvania State University	University of Wisconsin
Ali Hortaçsu	2011	University of Chicago	Stanford University
Eric Verhoogen	2011	Columbia University	University of California, Berkeley
Fiona Scott Morton	2011	Yale University	MIT
Gautam Gowrisankaran	2011	University of Arizona	Yale University

TABLE A6: OPTIMAL ORDER OF JOURNAL SUBMISSIONS

Case	Character	Years of Retirement (T)	Discount Rate (r)	Optimal Order
1	Professor Seeking Prestige	21	0.0822	<i>AER, Econometrica, REStat, JPE, IER, SJE, QJE, EI</i>
2	Assistant Professor Seeking Prestige	32	0.0822	<i>AER, Econometrica, REStat, JPE, IER, QJE, SEJ, EI</i>
3	Professor Seeking Readers	21	0.0822	<i>SEJ, AER, JPE, Econometrica, REStat, QJE, EI, IER</i>
4	Assistant Professor Seeking Readers	32	0.0822	<i>SEJ, AER, JPE, Econometrica, REStat, QJE, EI, IER</i>
5	Impatient Assistant Professor Seeking Prestige	32	0.1800	<i>REStat, AER, Econometrica, JPE, IER, SEJ, QJE, EI</i>
6	Patient Assistant Professor Seeking Readers	32	0.0600	<i>AER, SEJ, JPE, Econometrica, REStat, QJE, EI, IER</i>
7	Professor Nearing Retirement Seeking Prestige	8	0.0822	<i>REStat, AER, Econometrica, JPE, IER, SEJ, QJE, EI</i>

Source: Sharon Oster (1980, p. 446).

TABLE A7: SUBJECT-RELATED *AER* ACCEPTANCE AND REJECTION BETWEEN 1969 AND 1980 WITH A FOCUS ON NUMBER OF SUBMISSIONS AND ACCEPTANCE RATE (IN PARENTHESES)

	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980
	Ratio	Ratio	Ratio	Ratio	Ratio	Ratio	Ratio	Ratio	Ratio	Ratio	Ratio	Ratio
General Economics and General Equilibrium Theory	15 (3/20)	4 (1/26)	8 (2/24)	36 (4/11)	22 (2/9)	80 (4/5)	8 (1/12)	14 (2/14)	0 (0/16)	83 (5/6)	22 (4/18)	15 (2/13)
Microeconomic Theory	14 (28/204)	8 (15/177)	16 (16/98)	20 (19/96)	6 (7/108)	19 (22/115)	21 (18/87)	27 (21/79)	16 (19/116)	21 (18/87)	15 (23/157)	23 (21/90)
Macroeconomic Theory	16 (6/38)	4 (2/47)	2 (1/56)	19 (6/31)	10 (5/49)	15 (6/41)	12 (9/75)	24 (11/45)	13 (8/64)	11 (5/46)	4 (3/69)	30 (14/46)
Welfare Theory and Social Choice	15 (12/78)	23 (18/79)	29 (15/52)	19 (12/64)	25 (13/51)	28 (18/65)	21 (17/81)	17 (7/42)	22 (10/45)	21 (10/48)	39 (12/31)	45 (19/42)
Economic History, History of Thought,	0 (0/3)	18 (3/17)	13 (1/8)	13 (1/8)	33 (4/12)	15 (2/13)	0 (0/6)	0 (0/5)	18 (2/11)	9 (1/11)	24 (4/17)	10 (1/10)
Economic Systems	33 (2/6)	0 (0/6)	7 (1/14)	33 (3/9)	0 (0/17)	10 (1/10)	10 (3/30)	0 (0/27)	0 (0/20)	26 (5/19)	4 (1/24)	17 (1/6)
Economic Growth, Development,	22 (14/64)	16 (15/96)	14 (8/57)	15 (8/52)	18 (8/44)	30 (11/37)	10 (3/30)	53 (10/19)	17 (5/29)	11 (5/44)	23 (5/22)	36 (8/22)
Economic Statistics and Quantitative Methods	13 (2/16)	50 (9/18)	43 (9/21)	21 (4/19)	14 (5/37)	29 (5/17)	25 (6/24)	11 (2/18)	39 (11/28)	3 (1/31)	42 (8/19)	38 (8/21)
Monetary and Financial Theory and Institutions	18 (11/62)	10 (8/83)	20 (13/65)	44 (31/71)	15 (9/61)	8 (7/85)	12 (8/69)	38 (18/47)	18 (9/49)	14 (7/51)	20 (9/46)	24 (9/37)
Fiscal Policy and Public Finance	13 (5/38)	0 (0/24)	20 (7/35)	27 (7/26)	8 (2/25)	0 (0/27)	21 (6/29)	11 (3/28)	7 (3/42)	8 (3/38)	14 (5/35)	24 (5/21)
International Economics	19 (17/88)	22 (20/90)	18 (14/78)	18 (12/66)	21 (17/81)	20 (12/61)	15 (11/72)	34 (14/41)	24 (14/59)	17 (11/63)	20 (16/82)	18 (14/80)
Administration, Business Finance		29 (4/14)	0 (0/31)	11 (3/27)	11 (4/37)	18 (6/34)	25 (4/16)	11 (2/18)	0 (0/21)	32 (6/19)	17 (2/12)	5 (1/20)
Industrial Organization	20 (2/10)	7 (2/27)	13 (8/61)	19 (7/36)	16 (7/44)	15 (6/41)	13 (4/31)	14 (4/28)	8 (3/39)	20 (9/46)	11 (5/46)	15 (4/27)
Agriculture, Natural Resources	9 (1/11)	11 (3/27)	8 (3/37)	21 (6/28)	9 (3/34)	24 (8/33)	18 (2/11)	30 (3/10)	8 (2/25)	0 (0/11)	21 (4/19)	0 (0/14)
Manpower, Labor Population	28 (12/43)	10 (8/78)	9 (7/77)	15 (13/68)	16 (15/96)	11 (10/94)	16 (18/112)	15 (10/66)	20 (19/97)	13 (12/90)	12 (11/92)	21 (12/57)
Welfare Programs, Consumer Economics,	8 (5/61)	22 (11/50)	15 (10/68)	11 (7/62)	19 (10/52)	20 (7/35)	4 (2/57)	30 (6/20)	31 (9/29)	26 (10/39)	23 (7/30)	35 (8/23)
Other	6 (1/16)	5 (1/22)	0 (0/31)	0 (0/20)	0 (0/1)							

TABLE A8: CORRELATION BETWEEN JEL CODES REPORTED IN *AER* PUBLICATIONS VERSUS CODES REPORTED ON FIELDS OF SPECIALIZATION (JOB OPENINGS).

JEL Codes	ρ
(A) – General economics and teaching	-0.255 (0.292)
(B) - History of economic thought, methodology..	0.0303 (0.902)
(C) - Mathematical and Quantitative Methods	-0.4216* (0.072)
(D) - Microeconomics	-0.3569 (0.1336)
(E) - Macroeconomics and Monetary Economics	0.4221* (0.0719)
(F) - International Economics	0.2108 (0.3864)
(G) - Financial Economics	0.3346 (0.1615)
(H) - Public Economics	-0.2755 (0.2537)
(I) - Health, Education and Welfare	0.2384 (0.3256)
(J) - Labor and Demographic Economics	0.0096 (0.9690)
(K) - Law and Economics	0.3526 (0.1388)
(L) - Industrial Organization	-0.6148*** (0.0051)
(M) - Business Administration and Business Economics; Marketing; Accounting	0.0308 (0.9003)
(N) - Economic History	0.3938* (0.0953)
(O) - Economic Development, Technological Change, and Growth	-0.1791 (0.4632)
(P) - Economic Systems	-0.2435 (0.315)
(Q) - Agricultural, Natural Resource: Environmental, Ecological Economics	0.0488 (0.8428)
(R) - Urban, Rural, and Regional Economics	0.4996** (0.0294)
(Z) - Other Special Topics	0.2923 (0.2247)

Notes: Data based on the Report of the Editor (published in *AER Papers and Proceedings*) from various years and the Job Openings for Economists (JOE). We used the years 1991 to 2009 excluding category (Y) Miscellaneous Categories because of the lack of available data (was absent from the papers and appeared only in JOE years). p-value in parentheses; the symbols *, ** represent statistical significance at the 10% and 5% levels, respectively.

TABLE A9: TECHNIQUE IN ARTICLES IN FIVE ECONOMICS JOURNALS

Years	Primarily Verbal Techniques (%)	Geometry (%)	Algebra and/or Econometrics (%)	Calculus/More Advanced Techniques (%)
1982–93	95	3	2	–
1902–03	92	1	6	–
1912–13	98	1	1	–
1922–23	95	1	2	2
1932–33	80	1	8	10
1942–43	65	8	6	21
1952–53	56	6	7	31
1962–63	33	8	13	46
1989–90		Primarily Verbal Techniques (%)	Algebra and/or Econometrics (%)	Calculus/More Advanced Techniques (%)
<i>American Economic Review</i>		10.3	37.4	52.3
<i>Journal of Political Economy</i>		8.7	26.1	65.2
<i>Quarterly Journal of Economics</i>		5.6	50	44.4
Articles with Empirical Content 1989–90	Percentage of Articles with Empirical Content			
<i>American Economic Review</i>	37.4			
<i>Journal of Political Economy</i>	52.2			
<i>Quarterly Journal of Economics</i>	45.8			

Notes: The table summarizes the results presented by George J. Stigler, Stephen M. Stigler, and Claire Friedland (1995: 342–343). Different year periods based on five journals: *QJE* since 1892, *JPE* since 1892, *AER* since 1912, *REStat* since 1922, and *Economica* since 1932.